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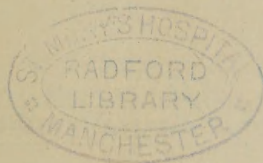
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THE DUBLIN JOURNAL

OF

MEDICAL SCIENCE.

JANUARY 1, 1878.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*The Life and Labours of Robert James Graves, M.D.*^a

By JAMES FOULIS DUNCAN, M.D.; ex-President, King and Queen's College of Physicians, &c.

MR. PRESIDENT,—I have been requested by the Committee appointed to erect a statue to the memory of the late Professor Graves to give a short account of their proceedings, now that their labours have been brought to a close, and to make a few remarks introductory to the proceedings of the day.

Some four years ago Dr. Hudson, who then filled the Chair which you now so worthily occupy, on the completion of his presidential career, requested the College to appropriate the then vacant space on your right to a memorial of the late Professor Graves, corresponding in character and dimensions to the other figures which now adorn this hall. This suggestion was at once cordially agreed to. Shortly afterwards a meeting was called of members of the profession and other persons likely to sympathise with the movement, when a large and influential Committee was appointed to carry out the design, Dr. Stokes and Dr. Hudson, with the late Sir W. Wilde, being appointed secretaries. And here, it is only justice to say, that the late Sir W. Wilde, whose untimely death every true-hearted Irishman must deeply lament,

^a An Address delivered at the unveiling of the statue of Dr. Graves in the Hall of the College of Physicians, Wednesday, December 19, 1877.

threw himself into the movement with all the enthusiasm which formed so conspicuous a part of his character, and that it was in a very great degree to his well-laid plans, his untiring energy, and his personal influence, that the requisite amount for effecting the object was collected. I need not add that the greatest assistance was rendered by every other member of the Committee. When the proper time arrived for selecting an artist to whom the execution of the work was to be entrusted, the greatest anxiety was felt that the person on whom the choice of the Committee should fall, should be one capable of producing a work fit to stand beside those already here, and not to suffer by comparison with the exquisite productions of the immortal Foley. After much deliberation they gave the commission to Mr. Albert Bruce Joy, of London, who, though young in years, has already, by numerous productions of his chisel, given proofs of genius and taste sufficient to justify the expectation that he will one day occupy a foremost position in his profession. He is an Irishman by birth, a pupil of Foley, and a son of a distinguished Fellow of this College. It is only right to add, in justice both to the Committee and to Mr. Joy, that this latter circumstance was not the point which guided their decision, but a personal examination of his works, some of which were exhibited a few years ago in the Royal Hibernian Academy in this city. How far the selection was judicious, this meeting, in a few minutes, will have an opportunity of deciding; but I shall be very much surprised if it is not considered, by those competent to form an opinion, that Mr. Joy has succeeded in producing an admirable representation of the original, in features, attitude, and expression; and that his work, viewed as a piece of art, is as beautiful in conception as it is exquisitely finished.

Mr. President, it is with very great reluctance I appear before you to-day. Much as I feel the honour of being permitted to take any part in the interesting ceremony before us, I am too sensible of my own inability to do justice to the occasion not to wish that the task had been assigned to some one more capable of fulfilling it in a becoming manner. I have been most unexpectedly called upon to take the place of my old and esteemed friend, Dr. Hudson, a place which is his by right, and which I know he would have filled to the satisfaction of every one. To him is due, as I have already stated, the entire credit of inaugurating the movement which has at length reached a successful termination.

Himself a highly accomplished student of the Meath Hospital,

when that institution had reached the zenith of its reputation, with Graves and Stokes for its Clinical Physicians, he enjoyed the best opportunities of estimating those lofty, intellectual, and personal qualities of his former friend and teacher, which raised Dr. Graves to the highest eminence in his profession, not only in this his native city, but throughout the world.

No one who was present on the last occasion when we were assembled within these walls for a similar purpose, when the statue on your left was inaugurated, can have forgotten the eloquent address he then delivered in praise of the literary labours and scientific attainments of Dr. Stokes—another of those illustrious Irishmen of whom our country and our profession are justly proud—and no one who remembers how admirably he discharged that duty then, can help regretting that he is prevented to-day from paying an equally high compliment to him who is the subject of panegyric on the present occasion.

It is not my intention to detain you or this meeting by any lengthened statement of the grounds on which the subscribers to this testimonial feel themselves justified in paying the present compliment to the memory of the late Professor Graves. The claims of this distinguished physician to the highest posthumous honours that his countrymen can bestow, are too numerous and too generally acknowledged to need any enforcement from me. If proof were wanting, we have it in the various distinctions conferred upon him during life, by different learned societies at home and abroad, and by some of the oldest and most celebrated universities on the Continent of Europe. But above and beyond all these outward symbols of authorised acknowledgment, there is a general *concursus* of public approval, a *fama clamosa*—not the less real even when it assumes no ostensible shape—which makes itself felt wherever it really exists, and which has long since accorded him a foremost place among the medical luminaries of modern times. A quarter of a century has passed away since his removal from among us, and I need not remind the distinguished audience that I now address that the reputation which he then enjoyed has lost none of its freshness or brilliancy in the interval.

And here I may be pardoned for quoting the testimony of one who of all others is most competent to form an opinion on the subject, because he is himself personally conversant with the topics on which he writes, and whose name cannot fail to command respect wherever it is mentioned—I mean Professor Trousseau.

His words are:—"For many years I have spoken of Graves in my clinical lectures; I recommend the perusal of his work; I entreat those of my pupils who understand English to consider it as their breviary; I say and repeat that, of all the practical works published in our time, I am acquainted with none more useful, more intellectual; and I have always regretted that the Clinical Lectures of the great Dublin practitioner had not been translated into our language. As Clinical Professor in the Faculty of Medicine in Paris, I have incessantly read and re-read the work of Graves; I have become inspired with it in my teaching; I have endeavoured to imitate it in the book I have myself published on the Clinique of the Hotel Dieu; and even now, although I know almost by heart all that the Dublin Professor has written, I cannot refrain from perusing a book which never leaves my study. Graves is an erudite physician—while so rich in himself he borrows perpetually from the works of his contemporaries, and at every page brings under tribute the labours of German and French physicians. Although a clinical observer, he loves the accessory sciences; we see him having frequently recourse to physiology, in the domain of which he loves to wander; to chemistry, with which he is acquainted, which he estimates at its true value, and to which he accords a legitimate place. He often reminds me of the greatest clinical teacher of our day—Pierre Brettonneau—an able physiologist, a distinguished chemist, a learned botanist, an eminent naturalist, who incessantly, in his lectures and conversation at the Hospital of Tours, found in all these accessory sciences, with which he was so conversant, those useful ideas and ingenious views which he subsequently applied with unusual felicity to the study of our art. Shall I now say what are, in Graves' work, the most remarkable and important lectures? To be just, I ought to indicate all in succession; there is not one of them, in fact, which does not abound in practical deductions; there is not one which does not bear the impress of that admirable and powerful faculty of observation which distinguishes, among all, the physician of the Meath Hospital."

Two brief but beautifully written sketches of Dr. Graves' life and labours have been given to the public, with which, doubtless, many of those who now hear me are familiar. One of these, from the gifted pen of the late Sir W. Wilde, was published many years ago in *The Dublin University Magazine*; the other was prefixed to a volume of his collected essays edited by Dr. Stokes, whose absence

to-day, owing to the state of his health, is a source of unfeigned regret to us all.

Each of these memoirs gives a short but luminous analysis of his character and achievements. They describe, with all the felicity of style for which these writers are conspicuous, his brilliant talents, his varied attainments, his abundant labours, his unwearied industry; they speak of his numerous and valuable contributions to practical medicine in its several departments of physiology, pathology, and therapeutics; they describe his eminent success as an author, a discoverer, a teacher.

They tell us, too, of his enthusiastic nature, his love of truth, his thirst for knowledge, his simple but lofty eloquence, his logical accuracy, his power of illustrating and enforcing the various topics he wished to inculcate, his courage in avowing and defending his opinions, whatever they were, in opposition to popular prejudice or old-established conclusions. In a word, they exhibit him as possessing a rare combination of intellectual feelings and endowments, which made him so conspicuous as an original thinker, a convincing reasoner, an efficient instructor.

Dr. Graves was eminently many-sided. The brilliancy of his undergraduate course, not confined to one department of study, which was marked by the unprecedented number of collegiate honours he obtained, proves beyond question that he would have attained to very high distinction in any path of life he had chosen to adopt, and the same universality of talent was equally conspicuous in the profession he actually embraced. It was, in fact, the foundation of his success, and the very quality, of all others, which fitted him for the Professorial Chair to which he was appointed. For while medicine, considered as a whole, is one and indivisible, it has many aspects, and the mental constitution which is necessary for the successful cultivation of one department is often unaccompanied by other qualities equally requisite in another. The practical physician, who delights to exercise his skill at the bed of suffering, frequently has no taste for the slow and difficult investigation of abstract scientific problems; while the man who shuts himself up in the laboratory or dissecting-room in the calm pursuit of learned study, shrinks with aversion from the drudgery of the hospital, with its anxieties, its dangers, its responsibilities. Then, again, each of these, however eminent in their peculiar path, may be deficient in the clear and ready utterance—the charm of voice, and manner, and speech, without which no man, however learned, can make an

efficient and attractive lecturer. It was Dr. Graves' good fortune to be capable of filling all these several positions with equal ease and acceptability. He was "*totus teres atque rotundus*;" and it was the power he possessed of making the information gained in one branch of his studies tell upon the others that made him so admirably fitted to be a teacher of medicine. This is the aspect of his life which shines out beyond all others as deserving of our notice on the present occasion; for while his numerous writings gained him reputation abroad, his popularity at home was that of a professor and a clinical lecturer.

Sir W. Wilde says of him:—"As a lecturer Professor Graves was endowed with peculiar capabilities. To a remarkable person he added great powers of arresting attention in the very outset of his discourse, which, by an almost startling impressiveness, he maintained throughout. His ideas were conveyed in a bold, fluent, and classic style. In his language he was always forcible and elegant; and, although frequently eloquent, he never sacrificed his subject for flowers of rhetoric, or lost sight of his text in the froth of a metaphor; for, whether discussing the investigations of others, or detailing the results of his own inquiries, he ever manifested the same critical acumen, the same powers of the most piercing analysis. But higher and nobler far, we rejoice to say, that with the privileges he enjoyed he forgot not, both in his lectures and addresses to the students, and in the presence of his professional brethren, whenever opportunity offered, to give the word in season; and while he taught his hearers what life does and where it ends, he likewise led their minds to contemplate with gratitude the Divine source from which it sprung."

One of the most important benefits conferred upon modern medicine with which Dr. Graves is to be credited is the complete change he introduced into the manner in which clinical instruction was given to the students attending the hospital. This consisted partly in distributing the care of the patients to a larger number of the advanced students than had previously been the custom, and partly in changing the scene of instruction from the lecture-room of the hospital to the bedside of the patient. The former brought each individual pupil directly into personal contact with the various types of disease, and by charging him with a sort of responsibility for the proper management of each case, made him take a greater interest in the various stages of its progress than he would otherwise have felt, and presented him with better opportunities of studying

its nature, observing its symptoms, and watching the effects of treatment. The mass of students accordingly, on the completion of their course, went forth into the world not charged with theoretic notions merely as to the work which lay before them, but rendered familiar in every possible way for the emergencies they might expect to meet in the pursuit of their profession. The latter converted what at best would have been an abstract dissertation upon the features of a case removed from view, into a practical demonstration under the very eyes of the audience. The importance of this method in developing the perceptive faculties of the pupils, forming habits of careful observation, of cautious induction and comparison, and of correcting errors arising from over-hasty assumptions, cannot possibly be exaggerated. What a different impression must be conveyed to the mind of a student when a lecturer merely describes, in the absence of a patient, the peculiarities indicative of the malady under which he labours—say, for instance, the aspect of his countenance, his attitude in bed, or his manner of respiration—compared with that which would be produced when the lecturer is able at the same time to illustrate his observations by actual inspection of the phenomenon commented on? Under any circumstances this is too obvious to be disputed, but it has become infinitely more so in proportion as we have been led to call in the aid of physical signs in the diagnosis of disease. How, for example, is it possible for a student to make himself acquainted with the various modifications of respiration produced in the chest by disease, except by personal auscultation of a patient in whom they occur, under the skilful direction of a master competent to discriminate between them, and to instruct him as to their nature, importance, and significance? And the same may be said of the rest of these signs. Dr. Graves then—as the first physician in these countries to recognise the practical superiority of this method of teaching and to introduce it into general use—deserves to be regarded as having a special claim to the character of a medical teacher.

And here let me pay a passing tribute to the dignity and value of the office of a teacher rightly understood. In a sense, all great men are teachers. They teach us by the nobleness of their example—by the light they shed upon the various problems of the day—by the influence they exert over the opinions of their fellow-men. Ministers of religion are especially and avowedly teachers. They expound the elements of divine truth; they enlighten the conscience; they give tone to the moral feelings of the community.

So, too, are our public statesmen who in the senate rule the destinies of the nation, and guide it in the course of enlightened liberty and progress. And so, also, are the high functionaries in the courts of law, who expound and enforce the eternal principles of right and wrong. All these are acknowledged as instructors of the race, and are held in reverence accordingly. But I speak not so much of these as of those excellent men, some of whom occupy a less prominent position in the social scale as instructors of youth. When any of these—from the village schoolmaster up to the heads of our universities—realise the proper ideal of their office, how high and noble it becomes; how important to the State; how invaluable to all who come under its happy influence. It is to take the raw material of the young mind at the most plastic period of its existence to shape its course—to mould its character—to form its habits—to fire it with lofty sentiments and a noble ambition, and to send it forth eager and prepared to discharge the duties of life with vigour and success. That Dr. Graves was not insensible to the responsibility of his position, considered in this light, may be proved by the following quotation from one of his published lectures:—

“ The teacher of clinical medicine, gentlemen, occupies in every nation a post of heavy responsibility. But when he happens to preside over the medical education of those who resort to the wards of a metropolitan hospital, when the metropolis is a British one, and the hospital destined to send forth annually practitioners to every quarter of the globe—to North and South America, to New Holland, to the Cape of Good Hope, to the East and West Indies, and to the countless isles which in either hemisphere are visited by the British flag—then, indeed, does that teacher become an instrument of good or evil to an extent which it is fearful to contemplate. He who gives instruction to a clinical class in Berlin, Stockholm, Vienna, or Paris, has much to answer for if he discharges not his duties with zeal and diligence; yet if he fails to make his pupils good practitioners, their errors, however deplorable, are circumscribed within comparatively narrow bounds, and limited in a degree to their own countrymen. But the British teacher sits in the centre of a circle far wider than Sweden or Prussia, or Austria or France; his pupils are to be met with practising in every climate, exercising their art in almost every habitable region of the globe—dispensing the blessings of health to all races of mankind—to the hardy white settlers of Canada, the aboriginal red skins of North

America, the negroes of Jamaica, the Hottentots and Kaffirs of Africa, and the countless tribes of Hindostan. In truth, gentlemen, the British teacher of practical medicine exercises an influence without parallel in importance and extent, and his opportunities of benefiting or injuring his fellow-men are incalculably great. If he neglects his duty, if he teaches erroneously, his negligence and his error in practice are multiplied indefinitely by those whom he ought to have better instructed; the scene of his guilt, for it deserves no better name, becomes fearfully enlarged, for there is no country so remote that it may not contribute victims to the incapacity of his pupils. But if, on the contrary, he works with zeal and diligence, if he labours conscientiously and perseveringly in performing the important task he has undertaken, a compensation awaits him to which scarcely any member of any profession can attain. The hero and the despot may extend a sovereignty over distant regions, may exert an unlimited control over millions of vassals, may dispense honours and rewards, or inflict punishments and death; they may, like Alexander, grieve at the narrow limits of a conquered world, and sigh for other scenes of glory; but they cannot chase away pain; they cannot bid the burning thirst to cease, or give back repose to the sleepless; they cannot impart feeling or motion to the paralysed, or sight to the blind; and, above all, they cannot imitate that almost God-like function of the healing art by which man is enabled to recall to his fellow-man reason long banished, and restore to society the helpless victim of insanity."

While Dr. Graves' eminent success as a writer, a discoverer, and a teacher, would have been sufficient, under any circumstances, to have warranted the erection of a statue to his honour, the eminent services he rendered to this city, as a place of medical education, renders it infinitely more so. It is difficult to realise now, when the Dublin School of Medicine—I use the term in its widest sense—is deservedly held in the highest esteem throughout the world, how short a time has elapsed since it emerged from the state of comparative obscurity in which it lay at the beginning of the present century, when its teachers were few in number and their reputation local in character and limited in extent. Dublin was not then the resort of students from foreign countries, or even from the sister-isle, as was afterwards the case; nor did they form the important class in the community that they do at present. The rise of the school was as rapid as it was remarkable. To Dr. Graves in a

great measure this happy change is to be attributed. Others there were undoubtedly—too numerous to be mentioned, and too well known to be forgotten—who laboured with equal earnestness and with great success in bringing about this much-to-be-desired end. But one of the foremost places—if not the very foremost—in this distinguished band must be given to the subject of our remarks, because of his transcendent ability, his indefatigable labours, and his justly-merited popularity as a lecturer. Sir W. Wilde says upon this subject:—"We well remember the stimulating effect the lectures had upon the minds of students, such as he described, who at four o'clock visited Sir P. Dun's to hear him. Then all weariness was forgotten—all languor vanished—the note-books were again resumed—the attention that had flagged at an earlier hour of the day was aroused by the absorbing interest of the subject and the energy of the lecturer—nay, more, the noisy bustle usually attendant on the breaking up of a lecture was exchanged for discussion upon the subjects treated of, or eager inquiries of the professor for the solution of difficulties, and the freshness of morning again came over the exhausted student's mind."

In one sense Dr. Graves did more than all his contemporaries to extend and perpetuate the character of Irish medicine by the establishment, in conjunction with Sir Robert Kane, of *The Dublin Journal of Medical Science*—a periodical now in the 46th year of its existence, which commanded public confidence and support from the moment of its publication, and which, after passing under various management, continues, in the hands of its present able editors, to make the literary labours of Irish practitioners accessible to their brethren all over the world. Previous to the establishment of this journal, the only medical works issuing from the Irish press were the "Dublin Hospital Reports" and the "Transactions of the Medical Association of the College of Physicians"—highly creditable productions undoubtedly, but which laboured under the disadvantage of having no fixed period of publication, and so failed in producing that interest in their contents which is necessary to secure a steady and extended circulation. The possibility of establishing a periodical of the class and pretensions in question, proved the fertility of the soil from which its contributions were to be raised, and, while it stimulated the authors to write, it secured an extended and attentive circle of readers throughout the world.

I cannot help remarking that there is something singularly appropriate in the circumstance that the names of Graves and

Stokes, so intimately associated during life, should be brought into equally close proximity before the eyes of future generations by the statues on your right and left. Colleagues in office, they were animated by the same noble ambition—to spread the reputation of the Irish School of Medicine over the civilised world, and to uphold the honour and dignity of the profession that they loved—an ambition unsullied by jealousy or personal considerations as to rival merits, which so often mars the finest human characters.

If I am asked what is the purpose aimed at by the subscribers in erecting this statue, I may be permitted to answer—it is to give a visible embodiment to those sentiments of pride and admiration which we feel towards one who was at once an honour to the country which gave him birth and to the profession which claimed him for its own—sentiments which had a real existence before the artist took the chisel into his hand, but the existence of which might be questioned or denied so long as they were destitute of any outward demonstration. It seems to me a matter of reproach that our public monuments are so few in comparison to what they ought to be, and that so many honoured names have been left without that visible acknowledgment to which they are fairly entitled. A better spirit is springing up amongst us, which, it is to be hoped, will not be extinguished until every one deserving a niche in the Temple of Fame shall have received the recognition to which he is entitled.

Let me, in conclusion, say we are not here attempting, by this act of ours, to rescue from oblivion a name that would soon otherwise be forgotten and pass away. No; that is not the idea we have had in so producing the likeness of him whose features have been engraved on the marble by your side. Were it so, the object would be as futile as the effort would be vain. He himself has achieved, by his own exertions, a renown which we can neither augment nor diminish. The true memorials of departed greatness are those which each man makes for himself—in the mark he makes upon the age in which he lives—in the impressions he leaves upon the institutions and destinies of his country—in the impulse he gives to the augmented knowledge and well-being of the world. Viewed in this aspect the reputation that Dr. Graves has left behind him bids defiance to the advance of time. This house in which we stand may be destroyed, the exquisite productions with which it is adorned may perish and be forgotten, but the undying memories of the eminent men whom they represent will survive the ruin, and

be held in grateful recollection by succeeding generations, as long as the English language lasts, or the Healing Art possesses a literature to boast of.

This College has now existed for upwards of two centuries, though the hall in which we meet was erected within the last fifteen years. It is a source of satisfaction to the members of the body that, in seeking for statues to adorn the building, we have not to go back to men of past generations, but we have found them among ourselves. Two of the illustrious men who have been thus honoured are still amongst us, and the two who have passed away are fresh in the recollection of almost every one present. This, I think, proves that our College has lost none of its vitality, and that we may safely predict for it a long and brilliant future; and that, when this generation has passed away—when future generations of young men enter on their career through this examination-hall—these statues, though silent, will act as monitors, and in imagination seem to say—“Work as we worked, live as we lived, strive as we have striven, and you will achieve success in life, and your names will be honoured after death.”

ART. II.—*A Few Practical Notes on the Surgery of the Bladder and Urethra, with their Diseases.* By ROBERT PERSSE WHITE, Fellow of the Royal College of Surgeons, and Licentiate of the King and Queen's College of Physicians in Ireland; one of the Surgeons to the Meath Hospital and County Dublin Infirmary; and sometime Surgeon to Jervis-street Charitable Infirmary.

BEFORE proceeding to cystitis in its various stages, which will be the chief subject of this paper, there will be brought forward a case of calculus in the bladder, with hæmorrhage and cystitis, which was treated by me by the process of crushing with the lithotrite.

Thomas F., aged sixty-six, residing at the North Wall, was admitted into Jervis-street Hospital on the 10th of December, 1867. Appears to be a fine, stout, healthy man; complains of severe pain in the bladder and penis; the pain is greatly aggravated by rough exercise, such as driving on a cart, &c.; he has a frequent desire to pass water, the current of which stops suddenly at times; the urine frequently contains blood: he has irritation and distress about the rectum, but no piles; there is no enlargement of the prostate gland to signify. He has been suffering from these symptoms for more than a year. I sought to examine the

bladder with a No. 8 silver catheter, but found the bladder so contracted and rugous that it was impossible to rotate the instrument. I then injected some tepid water (having first drawn off a couple of ounces of urine), but could only inject between three and four ounces, as he could not bear the pain of any more. No stone could be detected, although the instrument moved freely.

14th.—Mr. Stapleton kindly examined him with me to-day. We drew off the urine, which was loaded with mucus. About four ounces of water was injected, and then with a steel sound the bladder was freely explored, but without result. I did not give up the idea that there was a stone somewhere, but I determined for the present to treat him for the cystitis symptoms, which were most manifest. He was put on *pareira brava* with *hyoscyamus*.

On the 21st he left the hospital suddenly and without leave, before I had time to sound him again, as I purposed. For a couple of days previous to this all symptoms referable to the bladder had ceased.

Jan. 12th, 1868.—Re-admitted to-day, with a slight return of the symptoms of cystitis, but a frequent desire to pass water, which induced him to return, for in every other way he was in excellent health.

15th.—Sounded the bladder again to-day; there was a good deal of urine in it. I used a No. 8 silver catheter, having altered the curve, shortening it and bringing the point more forward. The bladder seemed much larger than when I examined him before. The instrument could be rotated freely, but failed to find a stone. I then depressed the end of the catheter well between his thighs, keeping the beak upwards, and on drawing well forwards I struck the stone with the tip of the beak; then putting on a sounding board all around, heard the click. Gave him a warm bath and ten grains of Dover's powder.

16th.—Examined him again to-day in presence of some of my colleagues, and found the stone in the same place above the pubes, finding it in the same manner as before. This time I drew the beak of the instrument along the stone for a little more than half an inch, so judged it to be a small stone, and I then tried to dislodge it, but did not think I had succeeded.

18th.—Dr. Stapleton examined him with me on this occasion, and we failed to find the stone in the former situation; but then seeking elsewhere found it in the fundus of the bladder, it having dropped down from its previous situation. The warm bath again, with Dover's powder at night.

22nd.—In company with Drs. O'Reilly and Stapleton, and several others of my colleagues, I proceeded to measure the size of the stone, when, having grasped it with the small lithotrite and measuring instrument of Charrière, and finding it only to measure five-eighths of an inch in diameter, and also believing from the touch that it was a soft phosphatic stone, I could not resist the temptation, and with strong pressure I broke

a piece off it, caught it again, and again a third time, each time crushing a portion. Gave him a hot bath, as he wished it, and the full dose of Dover's powder at bed-time.

24th.—Caught two small portions of the stone to-day with Civiale's small lithotrite, and crushed them then; found a large-sized piece behind the prostate, and crushed it, and then washed out the bladder with Mr. Clover's apparatus. As before, the warm bath and Dover's powder at bed-time.

26th.—In excellent health; pulse 64; has complete ease. Determined not to touch him again until the 29th.

29th.—Since last report he has had no pain or distress of any kind; very little of the *débris* has passed from the bladder. To-day I introduced the same instrument, having, as before, previously injected six ounces of tepid water, the urine having been drawn off. At this sitting I first caught two small pieces and afterwards two larger pieces of stone, one fully three-eighths of an inch in diameter. All were crushed, and then the bladder was washed out with Clover's apparatus, this time bringing away a good deal of *débris*.

30th.—Finds himself quite easy and free from pain.

Feb. 3rd.—No change since last report. A little detritus has come with the urine. On this morning I again operated, crushing three small pieces, and then found two large pieces and crushed them; the largest broke with a sharp click, and the resulting *débris* which came away in the lithotrite was oxalate of lime—all previous detritus having been phosphatic in composition.

8th.—No change since; passed some detritus; the urine is now almost free from mucus; he is in excellent health. The pulse never rose above 72. This morning I crushed three small pieces, each of about three lines in diameter; these seem to be the last pieces remaining.

17th.—Sounded him carefully, but could not find any trace of stone or grit.

18th.—Exit.

I had the opportunity of seeing this man two years after the above date, when he stated that he had ever since been quite free from all symptoms connected with the bladder. I saw some of his urine; it was perfectly healthy.

Cystitis arising from the use of a Catheter in a Case of Injury of the Spinal Cord at the Fourth Dorsal Vertebra.

In this case considerable irritation occurred within a day or two after a catheter had been passed, the bladder being unable to empty itself. The injury to the spine had produced complete anæsthesia

of the lower limbs, from the lumbar region down, with that peculiar muffling of the feet. The bladder got into that state which has been recently called "stammering," and was unable to more than half empty itself.

The urine being high-coloured and very fœtid, I thought it well to use the instrument, and to draw off the remaining urine each day; cystitis was rapidly set up. The ordinary treatment going the whole round of *pareira b.*, *buchu*, *uva ursi*, and all else, including washing with various substances in the lotion, gave but momentary relief. The distress in the afternoons became intense; every half hour or less, intense spasm of the bladder would come on. In the lower part of the abdomen the bladder could sometimes be felt like a cannon ball or a uterus contracting after child-birth; then after a time a little scalding urine would come, but it only brought another form of suffering, for the pain in the prostatic portion was so severe that it seemed like fire. This state continued with intervals of slight repose, generally arising from the use of the hot bath and free drinks of citrate of potash in effervescence. Opium did no good, only increasing the scalding character of the urine, which, when fresh caught, was very acid, but rapidly decomposed, becoming ammoniacal, depositing a quantity of ropy mucus, and also a large amount of phosphatic sand—sometimes a small teaspoonful in the four-and-twenty hours. The distress at night now became fearful; for nearly two years he had but little rest at night, often up from ten to twenty times, disturbed by the agonising tenesmus. Such was his state when, in the early part of the year 1875, I found him in his study one afternoon in a fit of agony.

Having failed hitherto in all routine treatment, the idea came into my mind that I would try a very weak solution of borax in very warm water. I had previously used twenty grains to the 8 oz. syringe of warm water, but without any good effect. On this occasion I only used four grains of powdered borax in 8 oz. of warm water. The bladder resisted and rejected the fluid forcibly; the first 3 oz. of the fluid washed out a quantity of phosphatic sand. I then injected the remainder; about an ounce was retained in the bladder this time. When I saw him the following morning he appeared strangely relieved. After the use of the borax he had no pain, no spasm, or desire to pass water for three hours, and then it came on the first moment with a slight tendency to spasm, and after that with perfect ease. That night he got several periods

of more than two hours of rest, and on the second night had only to arise three times. It would be useless to detail the every-day treatment of this case, but for some weeks I continued the washing out daily with the weak borax lotion, omitting all other treatment. As time went on he began to be able to retain his urine for the whole night until the early morning, and having seen him very lately, I find that he is able to sit at table till twelve o'clock at night in perfect comfort, and to pass five or six hours each day without a thought of suffering. At times he passes sand, and there is great scalding, but the borax wash at once relieves him.

Throughout the latter part of this case no treatment was used except the wash, except on three occasions at long intervals, when great quantities of sand were passed, and the pain in the prostate portion of the urethra became very severe. It was relieved and in fact cured in a few minutes by the injection of a sixty-grain to the ounce solution of nitrate of silver injected into that part of the urethra with a silver catheter, using one with small holes, and a glass silver-mounted two-drachm syringe. A moment of smarting followed, but the relief was amazing.

John N., a labourer, aged forty, was admitted into the Meath Hospital on March 4, 1876. Has a close stricture of the urethra, a No. 2 only being got into the stricture, but not passed in. This was kept in the stricture as long as he could retain his urine, about two hours. He complains of frequent micturition, the tenesmus being extreme, urine coming very slowly, thready, and full of mucus, sticking to the sides and bottom of the vessel; and loaded with a white granular sediment. There was intense distress at night; had to get up from twelve to twenty times to try and ease himself.

March 5th.—A No. 2 catheter was passed and kept in for four hours. On the previous evening he had had a hot bath and ten grains of pulv. ipecac. co. at bed-time.

7th.—A No. 5 catheter was passed, and his bladder was washed out with a warm solution of four grains of pulv. sodæ bibor. in 8 oz. of water; about 2 oz. of a similar fluid was introduced into the bladder after the washing, and he was to retain it there until the next call to pass urine. Warm bath and ten grains of Dover's powder each night.

8th.—Better. Urine much clearer; only passed water three times last night; the mucous deposit has greatly lessened. To be washed out each day with a wash the same as that used on the 5th.

9th.—On this day a No. 7 catheter was passed. He now has only to get up twice during the night. All pain and tenesmus have passed away completely. He passed his urine in a good stream. The washing to be

continued daily for some time. The catheter was used in increasing size up to No. 12.

April 24th.—He left hospital on this day perfectly restored in health, all the mucus and all phosphatic deposit having long since ceased.

The next case which came under my notice was on April 3. A man named W. K. was admitted; he was suffering from some contraction of the urethra, but on searching with a catheter I found I could pass a No. 5. His chief ailment I then found to be cystitis of two years' standing, and now become chronic. Great distress and frequent passing of urine at night. The night's urine contained the usual deposit of mucus adherent to the vessel, and also purulent deposit with phosphates. It is hardly worth detailing the daily work in this case. The routine as in the last one was followed; in a few days, with instruments graduated, regularly passed on each, pulv. Dov. and the hot bath, he improved quickly. On the eighth day after admission I got in a No. 10, and he never passed that, as any attempt with a larger only produced irritation. All mucus and discharge passed completely away, and he left hospital on 29th, quite cured, keeping himself quite right by passing a catheter each night.

I may now remark that from the earlier cases which for years I had seen in my own and in other men's practice, and from consultation with many of my brethren, I had come to the conclusion that the whole "role" of treatment hitherto practised was of little avail. I had tried pareira brava by gallons, uva ursi, copaiba, nitrate of potash, lotions and washes, alum, nitrate of silver, a two-grain solution—all had failed. I had seen no case cured, but few relieved, and then only by citrate of potash in large doses and with abundance of water. Hyoscyamus, opium—all useless. But—as these cases show—a very simple line of treatment (too simple for some to follow) gave the relief too long sought for, and most *complete relief*. The only adjuncts necessary in these cases are—1st. Temperance of life; 2nd. The occasional washing out of the bladder with the weak borax lotion whenever any symptoms of irritability come on.

I have cases now in progress which I will, I hope, bring before the profession. I now conclude with the case of a lady who has been a great sufferer, and permits me to add her case to those which have gone before. It is now many months since I ceased attending her, and on visiting in her house lately, in consequence of the illness of some of her children and grandchildren, I found her in perfect health. She said she never had any distress or suffering

since last I had seen her—that she was in perfect comfort and health.

A few days since this lady wrote to me, sending me the following account, more graphic than anything I could write:—

“For more than four years I suffered severely from inflammation of the bladder; but as the symptoms were for some time more general than local, I attributed my illness to a weakness of the digestive organs, or a general break-down of the system. I had frequent attacks of the stomach, pain in the head, a drowsiness in the day-time which I found it impossible to shake off, whilst my sleep at night was painful and unrefreshing, accompanied as it was by distressing dreams, out of which I frequently started in a state of nervous tremor which seemed to shake my whole system. As the disease advanced, the more local symptoms made themselves felt, and to my other sufferings were now added a sensation as if of the bearing down of the abdomen, accompanied by an internal inflammatory pain which the slightest fatigue or bodily exertions augmented to an intense degree. My urinal discharges were infrequent and evidently impeded, and the odour arising therefrom was extremely offensive. I tried simple domestic remedies, such as hot stupes, poultices of linseed meal, and also cold applications. The poultices gave me temporary relief; but as it was only temporary, I called in the aid of Dr. White, of Harcourt-street, who has attended my family and myself for several years, and who prescribed for me in the earlier stage of my disease, when I fear I was not sufficiently explicit in describing my symptoms. I now, however, carefully and minutely made a statement of all my sufferings and sensations, and he perceived the nature and locality of my disease at once, and commenced a course of bi-weekly surgical treatment, which gave me immediate relief, and eventually effected a complete cure. Although it was four months before I was perfectly restored to health, I consider it a rapid and wonderful restoration, when I reflect upon the deep-seated nature of the disease as well as my advanced age. Having learned that Dr. White was preparing a paper on the treatment of diseases similar to mine, I gladly and gratefully communicate these facts, not only as a well-merited proof of his surgical ability, but as a practical proof of gratitude to Him who has allowed that skill to prove so effectual in assuaging human suffering—trusting that it may, in however slight a degree, strengthen Dr. W.’s arguments in favour of the adoption of a system of surgical treatment which, for painlessness and effectiveness, I feel convinced cannot be surpassed.—J. C.”

There was no variety in her treatment, except what will appear in subsequent cases. The use of solut. atropiæ $\text{m}j$, horâ somni pro re natâ, very rapidly allayed all dribbling.

In an early number of this Journal I propose to give several cases which have been under my care, and some of which, whilst I now write, are under treatment, and which give every promise of a successful result.

ART. III.—*On Calcification of Adipose Tissue.* By EDWARD H. BENNETT, M.D.; Professor of Surgery in the University of Dublin; Surgeon to Sir Patrick Dun's Hospital, &c.

My object in publishing this note is to place on record an account of a pathological condition which, so far as my information reaches, has not been hitherto noticed by others—namely, the occurrence of calcification of adipose tissue.

The clinical phenomena attendant on the affection are sufficiently interesting, although not of grave importance. Before I enter on the description of these, or of the microscopic characters of the altered tissue, I must guard myself against the danger of being misunderstood in the use of the term “calcification of adipose tissue.” I use it in the same sense and with exactly the same limitation as it is commonly used in regard to other tissues—cartilage, for instance. I mean that the change is seated in the connective basis of the tissue, not in the contents of the cells. Viewed in the general aspect, there is no novelty in the observation that connective tissue is liable to calcify; but I am not aware of any observation having been as yet recorded of the existence of the change under the conditions, and with the microscopic features, which I shall presently describe.

In the subcutaneous tissue of the anterior aspect of the leg in elderly women, small hard bodies may be often observed—flattened on the superficial and deep aspects, circular in outline, the largest about one-fifth of an inch in diameter, the smallest mere grains. These bodies are freely movable on the deeper tissues and beneath the skin, and are arranged with a rough symmetry in the two limbs; if there be but one or two in a limb, the finger carried over the corresponding part of the opposite limb readily detects even the single specimen. When they are numerous, their symmetry is similar to that of cutaneous eruptions, not absolutely exact, but very nearly so. They occur in thin-skinned, pale bodies, and so can generally be seen before their detection by the hand. I have never seen them associated with varicose veins, or with skin eruptions, or

ephelitic markings on the legs. They are most commonly seen in the limbs of the pauper subjects in our dissecting-rooms; but I have seen them in the living also in hospital. They are not the seat of any trouble or pain to the patient, and pass unnoticed by them until attention is directed to them by the surgeon. I have never seen them in the male. In my early examinations of them, I sought for small veins, or varices, as their seat, under the impression that they were phleboliths. I next searched for a lymphatic vessel passing into or connected to them, being still impressed with the idea that they were the result of some vascular obstruction, but I failed to find any anatomical support for such idea.

The close relation of the bodies to the subcutaneous fat, always embedded in the lobules and intimately connected to them, made the search for minute vessels—such as lymphatics—difficult; but I was satisfied that such relation did not exist. I had heard these bodies described as of gouty origin, but their colour (a dark yellow) and hardness, even without chemical examination, set aside this theory, for tophi are white and most friable.

Being foiled in the attempt to explain their origin in some vascular obstruction, and not entertaining the gouty theory at all, I resolved, when next I met them, to make the microscopic examination with care. Some time elapsed before opportunity occurred, as my means of observation, so far as the dissecting-room affords it, are less frequent than in former years. Last summer I obtained abundant specimens in one subject and a few from a second, in each case absolutely agreeing with the observations as to the mode of occurrence, &c., which I had previously noted. From these I obtained the following results by microscopic examination:—

I made a thin section of the centre of one of the largest of the bodies dried. Adopting the ordinary process for hard, brittle substances, I polished a flat surface on one face of a section made with a fine saw through the centre of the body, and cemented it to a glass slide with old Canada balsam; I then ground away the structure until I obtained a fine transparent section. In this process I learned that the densest part of the structure was at the circumference—the most open and friable at the centre. Examined, after completing the mounting with fluid Damar varnish, the pattern of the thin circumferential part was clearly seen to be that of ordinary condensed connective tissue, forming a capsule for the body, calcified. In it the usual irregular lacunæ, dark by transmitted light, due to gaps in the structure, were readily seen; septa

from the capsule passed irregularly through the structure, themselves calcified and showing lacunæ similar to the outer layer. The arrangement of these parts was such as every one familiar with the microscopic appearances of the compound tissues would recognise as that of the envelopes and septa of subcutaneous fat. In the intervals enclosed by these calcified envelopes and septa the mass of the structure appeared arranged strictly in the pattern of the fat cells, the intercellular substance being calcified and breaking with a brittle, glassy fracture. Fearing error in a single observation, I repeated the process with several specimens, and obtained results exactly similar. I next macerated a fresh specimen in a weak picric acid solution, to which a minute quantity of hydrochloric acid was added. I established in this way the fact that the earth salts were deposited in the connective tissue forming the capsule and septa of a lobule of adipose tissue, and in the intercellular structure of the fat cells. The decalcified tissue presents the pattern of ordinary fat, with only the exception that the structures out of which the earth salts have been dissolved are thicker than in the healthy tissue. One point further only remains to be stated—the position of the calcified body in the fat lobule; this I have always found to be marginal, never central. I have never seen any such alteration as I have described in lipomata or in any part of the body except that mentioned above.

THERAPEUTIC USES OF SALICIN.

DR. MACLAGAN—whose original paper on the use of salicin in acute rheumatism may be said to have initiated the most generally successful mode of treatment for that disease at present known—publishes in *The Practitioner* for November his experience of the same drug in some other ailments. He has found it relieve *neuralgia*, especially those cases in which the pain is more or less periodic. In his hands, and given in large doses—eighty grains in twenty-four hours—has succeeded with him in cases in which either quinine failed or could not be taken owing to the disagreeable effects produced. Cases of *cold in the head* have, in his experience, been speedily benefited by frequent doses of salicin. Rapid relief has also followed, he has found, the administration of salicin in twenty grain doses every hour in *acute lumbago*; and in two cases of *hay fever*, one treated by salicin, the other by salicylate of soda, each taken in doses of twenty grains every two hours, relief was experienced while the drug was being taken, but the symptoms returned when it was omitted.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Clinical Records of Injuries and Diseases of the Genito-Urinary Organs. By CHRISTOPHER FLEMING, A.M., M.R.I.A.; late President and Fellow of the Royal College of Surgeons, Ireland; late Surgeon to the Richmond Hospital; Visiting Surgeon to Steevens' Hospital, Dublin; Corresponding Member of the Société de Chirurgie de Paris, &c., &c. Edited by WILLIAM THOMSON, A.B., M.D., Fellow and Examiner, Royal College of Surgeons, Ireland; Surgeon to the Richmond Hospital, Dublin. Dublin: Fannin & Co., Grafton-street. London: Simpkin, Marshall, & Co.; Longmans, Green, & Co. 1877.

THE Dublin School of Surgery, ever foremost in the struggle for practical distinction, is not likely to have its position weakened by an observer of Mr. Fleming's reputation, whose indefatigable industry in the pursuit of practical knowledge is so well known to his friends. We received this book of his with much gratification, and although we have been compelled to differ with him on some matters, we nevertheless consider that he has done good service in its production.

The work commences with some useful observations on the "Pathology of the Urine in its Relation to Surgery." Amongst these observations will be found a caution, probably intended for the student, to avoid all sources of fallacy in the examination of specimens of urine. There is often, he remarks, carelessness in conducting such investigations, and hence erroneous conclusions are arrived at. Strange to say, he does not allude to a frequent source of fallacy—the secretion of the urethra contained in the first flow of urine, and therefore the urine that first escapes should not be collected.

Plate I. represents the urine jars Mr. Fleming uses. These, nine in number, are delineated with contents composed of uric acid and its compounds, oxalate of lime, phosphates, blood, pus, mucus, blood and mucus, with crystalline deposits. The engravings

are artistically arranged, are pretty in their way, and we hope may be of use, although poor substitutes for the products they represent.

We are reluctantly compelled to dissent from Mr. Fleming in the opinion that the student will "learn the physical properties of the urine in health and in disease," by "consulting those authors who have of late so successfully investigated the subject of urinary pathology." We, on the other hand, are under the impression that specimens of the fluid of health and of disease are essential for the acquirement of a practical knowledge of their characters. Indeed he himself subsequently admits this. His description of the contents of glass 1 (Plate I.), to our mind, is obscure. He begins with the statement that they are "almost peculiar to childhood," and then shows that they have a much wider range; for who, he asks, has not daily observed deposits of urate of "ammonia or soda?" In the description of Fig. 2 of the same Plate he mentions the rapidity with which uric acid crystals appear in specimens of urine containing oxalate of lime, but we may observe that the reverse change occurs frequently. It is rather common to find oxalate of lime octahedra apparently replace uric acid crystals, probably from the decomposition of the urates after the urine has been voided—a question to which the late Dr. Aldridge, of Dublin, drew special attention.

Mr. Fleming seems to have seen more cases of malignant disease of the bladder—whether he means carcinoma he does not say—than has fallen to the lot of some of our most experienced writers on its surgical diseases, cancer of the bladder being, according to them, comparatively speaking, rare. He states, for example, in the description of Fig. 5 (same Plate), that "it indicates that bright, prune-juice tint, often pathognomonic of malignant disease of the bladder." All the figures of Plate II. refer to cases of stone in which lithotomy had been performed. Some of these figures are characteristic as well as artistic.

Whether it was from true incontinence or from mere overflow of urine the subject of Case VI. suffered, we are doubtful. At all events, in this case, called by the author "Laceration of the Penis and of the Urethra," the penis, while in a state of erection, was seized by a prostitute, who gave it a twist, which reminds us of recorded cases of persons who ruptured the penis during chordee, by placing the organ on a table or some other resisting structure, and giving it a sharp blow. In some of these cases the hæmorrhage was abundant; in others there was retention of urine.

Rapidly fatal blood-poisoning and traumatic stricture of the urethra were also observed.

Case VII. is an instructive one; in it the urethra was completely severed by a fall, the penis having come in contact with the edge of a door of a kitchen range.

The negative mode of treating fracture of the pelvis recommended by Mr. Fleming is suitable probably for many of the cases of this accident. Of it he speaks thus:—

“As in certain cases of fractures of the ribs, mechanical compression of the thorax cannot be tolerated, so likewise in fractures or other injuries of the pelvis, the agony resulting from the adoption of any constrained provision for adjustment is frequently extreme, and its advantages are very questionable. In such cases I believe that if attention be paid to the proper position of the pelvis, measured by its symmetrical relations to the lower extremities, all that is desirable in local treatment will be accomplished.”

That he did not follow this practice indiscriminately, the description of Case XVI. shows, for he mentions that “at the end of two months, with the usual provisions for absolute fixity of the pelvis by bandage (the thigh being kept permanently extended, as in the ordinary fracture of the shaft of that bone), the man left the hospital with excellent power of progression.”

Case XXI. recalls to our memory the article on foreign bodies in the rectum in Cooper's Surgical Dictionary. In this case a large bottle, introduced into the rectum by the patient, was accidentally broken, and the pieces were subsequently removed by Mr. Fleming. He asks “whether the bottle could not have been removed more satisfactorily by other means than by those adopted,” but he has not favoured us with any suggestion thereon.

It was scarcely necessary to introduce Case XXII. into this book—a case of prolapse of the anus and rectum. Being, however, a clinical record of the author's practice, it was possibly excusable. In this case he applied nitric acid, with very successful results, to the mucous surface of a protruded rectum, but this treatment is not new, having been practised by many other surgeons. The actual cautery, it is almost needless to say, has been also used in cases of the kind. The thermo-cautère of Dr. Paquelin is an excellent apparatus for applying the heat.

Case XXXIII. is of much interest; in it the integument of the penis was completely torn through near the pubes, and dragged forward by a comrade. Mr. Fleming remarks that, had he seen

the case in a less congested state than when it was first presented to him, he "should have tried to secure the integuments *in situ*, and promote their union. But he says also that "it would appear to" him "better in this class of case to anticipate any gangrenous action, and its accompanying fever, by at once removing the whole detached and shrivelled integuments of the penis." It appears to us that we should be governed in our proceedings by the amount of injury and separation the integuments have undergone. The shrivelling may be very deceptive, and lead to an unnecessary waste of integument; the skin of the penis having a great tendency to retract and shrivel, without being injured, as witnessed after its division in the operation of circumcision. Be this as it may, Mr. Fleming's case, although in a lesser degree, resembles one that was mentioned by Dr. Archibald Jacob at the meeting of the Surgical Society of Ireland, held on the 19th April, 1872.^a It was a remarkable case that came under Dr. Jacob's observation when a student, and which, he remarked, was valuable as proving that these very dangerous injuries were by no means uniformly fatal. When he was serving his apprenticeship at Maryborough, he saw the case of a young man whose genital organs had been seriously injured. He had been returning home from a fair, driving an ass's cart, and from some extraordinary notion that entered his mind, he tied the rope reins of the ass round the penis and scrotum. Jerking on the reins, the animal ran away; the young man fell off the cart, and the reins being strongly constricted on the penis and scrotum, tore away the whole of the testes and the whole of the integumental covering of the penis, leaving the penis itself in a condition resembling a goose's neck just stripped. The man recovered after a lengthened illness, and with an extremely mutilated organ, and lived for many years afterwards.

Our idea of an operation *de convenance* does not coincide with that of Mr. Fleming, who describes as such an operation of his on a man for the relief of intense inflammation of the prepuce, accompanied by enormous swelling threatening destruction of the penis, and possibly the life of the patient. It is our impression that an operation *de convenance* is, in strictness, an operation to be performed for the removal of deformity, either congenital or the result of disease, or of accident, in the seat of which all inflammatory action has altogether ceased. Some writers, we are aware, give it a wider range, but this would scarcely embrace Mr. Fleming's case.

^a Medical Press and Circular, 8th May, 1872. Vol. LXV.

Mr. Fleming has not forgotten the familiar precept that when about to divide the prepuce we should take care that the director does not "pass inadvertently into the urethra, and the glans penis be included in the section." He has known this to happen, and we also have both heard and read of its occurrence. To avoid it, a knife constructed on the principle of the knife (attributed to Blandin) should be preferred to the bistoury and director used by Mr. Fleming. Our readers will probably agree with us in thinking that Mr. Fleming does not sufficiently distinguish between incontinence of urine and mere overflow of this fluid—a distinction thoroughly practical, and upon which some of our most discriminating writers lay great stress. Thus, in his observations introductory to the case he relates at page 102, he speaks of incontinence accompanying retention. We, on the other hand, would rather consider the discharge of urine in a similar case a mere overflow from the bladder. In true incontinence there is no retention; the bladder, in fact, resents the presence of the urine and expels it.

We trust that the inception of the following observations on catheterism has not had its origin in Irish surgery. "The urethra," Mr. Fleming remarks, "is sometimes treated with unaccountable roughness, and too often coarse manipulation in the use of instruments appears to be the measure of the dexterity and skill of the surgeon. He is frequently rash in having recourse to any operative interference whatever as regards the urethra; he is careless in the selection of a suitable instrument; he is reckless in its direction and in its management, and particularly as regards the position in which he should place his patient for its introduction." Mr. Fleming appears to have been an eye-witness of the following practice:—

"The curved stilet was attempted to be passed through the catheter while the latter was impacted in the stricture. Most agonising pain and severe hæmorrhage followed on this occasion; the presence of the instrument could not be tolerated, and it was necessarily withdrawn, when the stilet was found pointing through the eye at the end."

In the observations on the selection of catheters there are some practical hints regarding stilettes which it is well to remember. There can be no doubt that "they are often too short, often too loose, and very often too tight." The two first qualities render such stilettes "very hazardous," and should be avoided by every purchaser. These observations, however, require qualification. If the end of a stilette, when sent home, does not appear within the

area of the catheter's eye, it must necessarily be very short. Indeed this relative shortness has been recommended for certain cases of difficult catheterism, to enable the end of the catheter to accommodate itself to the urethral curves; but if the stilette is a little longer, and appears in the area of the eye, the length not being sufficient to enable it to clear it, and go home to the end of the instrument, it is then of that dangerous length to which Mr. Fleming alludes, and may at any moment, when in use, pass out through the eye. The author attributes the auto-passage of instruments into the bladder to a "vermicular action of the urethra, and perhaps additionally by a suction effort from a distending bladder exercising a sort of traction whereby the instrument may easily pass backwards." This vermicular action hypothesis is not altogether new, and was, not very long ago, brought under the notice of the Surgical Society of Ireland by Professor Macnamara, of whose name Mr. Fleming omits all mention. We can scarcely believe that the suction theory is a satisfactory one. This accident, according to our observation, does not happen unless the posterior end of the foreign body has, from some cause or other, entered the urethra. When this takes place the canal resumes its fissure from between the orifice and this end, which it thus pushes towards the bladder. Moreover, the urethra being a more or less spiral rather than a straight passage, may have some influence in this retrograde movement. Is it possible, as Mr. Fleming states, that a certain instinctive action is "inherent in the urethra to direct" this course of the foreign body? He observes:—"We must not overlook the fact that, as in the œsophagus, a certain instinctive action is inherent in the urethra to direct its course." We confess ourselves to be so dull as not to see that, because food passes along the œsophagus, and its refuse escapes finally at the anus, the similitude between the action of the œsophagus and that of the urethra is very striking. Be this as it may, Mr. Fleming's practical caution is judicious, that "in no instance should an attempt be made to test the site of the foreign body with a sound, catheter, or other instrument, or to remove it with forceps or other contrivance through the tract of the urethra, until every precaution is adopted to prevent its passing into or towards the bladder." He illustrates his modification of Hunter's urethral forceps, and gives the particulars of an instructive case in which he removed a calculus from the urethra with its assistance. We do not think that he has clearly shown that urine was extravasated in the child whose case is

mentioned at p. 156. We are more inclined to attribute the tumefaction to causes other than infiltrated urine and its consequences, the bladder having remained "fully distended," while the extravasation was supposed to be progressing. Indeed retention recurred after withdrawal of the catheter from the bladder. We are sustained in this view by the fact that "not a trace of sloughing or suppurative action was to be detected in the site of the extravasated urine throughout the whole progress of the case." Although we agree with Sir Benjamin Brodie that some residual urine may be found in the bladder after rupture of the urethra and urinary infiltration, we are not prepared to assent to Mr. Fleming's view of this case, and have no doubt, as he remarks, that a similar one has not been recorded. He seems to consider that when it is necessary to incise the urethral orifice for the removal of a calculus, "*the division in front through the glans is preferable to that towards the frenum.*" We are inclined to believe, with him, that by so doing "there is less mischief done, and it is more manageable."

Case XLVI. might have been more fully elucidated. Who the lithotritist was, Mr. Fleming does not state, but he saw the patient with "violent hæmorrhage," and in a "terrible paroxysm of dysuria," and he tells us that after four sittings he "was free from any remaining symptoms to indicate the presence of any portions of *detritus* of importance." *Detritus*, however, remained, and yet we are told that "the treatment of the surgeon in many such instances should be expectant, at the same time that it must be watchful." Some lithotritists would possibly in such case take steps, in a few days after the sitting, to remove the remaining *detritus* rather than wait for symptoms to result from its presence in the bladder.

An objection has been advanced against Allarton's median operation by Mr. Fleming, who is "rather sceptical regarding the capability of even the most accomplished anatomist to strike the healthy membranous portion of the urethra from the perinæum so very accurately as is asserted, so as not to implicate the bulb more or less." Sir Henry Thompson has anticipated him on this point. Speaking of Allarton's operation, he says that he has arrived at the conclusion that there is quite as much bleeding as in the lateral operation. He attributes this to the bulb as a large artery to all intents and purposes. "You cut," he observes, "into that spongy tissue—not in all cases, but in some—and there is as much bleeding as if you cut the artery of the bulb, and more difficulty in controlling it. The bulb must be cut more or less in the median

operation." We confess to a temporary bewilderment on reading the following passage:—"The *post mortem* examination revealed universal pleural adhesion, and obliteration of the pericardial sac by the same cause." We cannot understand how universal pleural adhesion could cause obliteration of the pericardial sac, and we suppose it is intended to convey that pericarditis previously existed, having for its result pericardial adhesion and sac obliteration.

The author recommends the surgeon to cut down at once, in acute prostatitis, on the capsule of the prostate, and divide it, but should the patient not submit to this treatment, he advises local leeching, mercury, hip baths, cupping the perinæum, and the use of anodynes. For our part we should prefer to reverse the order of practice here laid down, and to give the milder precedence to the more severe means.

Mr. Fleming attributes the condition of the limbs and kidney observed in Case LVIII. to phlebitis, or true inflammation of veins; but it is open to question whether the coagula found in the affected veins were the result of true inflammation of these vessels. Although inflammation of the outer walls of veins is admitted by all pathologists, that of the lining membrane has been denied by some observers. At all events, in a work dedicated to students, they might have been given some information on this debated point.

It would have been satisfactory had Mr. Fleming mentioned what means were employed in the examination of the liver of Case LIX. It appears to us marvellous that, in a liver still weighing two pounds, "all traces of hepatic cells . . . had disappeared." Unless every particle of the liver had been examined, we should be loth to assent to an assertion so sweeping, particularly as the author has withheld from the reader the symptoms of the patient's last illness. He gives the particulars of an interesting example of that rather rare growth, polypus of the bladder, in which frequent recurrences of hæmaturia were observed, the analogy to menorrhagia, the result of certain uterine growths being remarkable. We should here state that, in his interesting account of hæmaturia and its treatment, he has not omitted to draw attention to the simulation of this form of hæmorrhage by various colouring matters eliminated by the kidneys, and to the deceptions that may be practised on the surgeon by the dishonest or the hysterical person.

The author considers the literature of stricture of the urethra to be so very copious he contents himself "by some general practical

observations, the result of "his "own experience." With regard to the situation of stricture, he considers that it may exist in what he calls "any tract of the urethra, with the exception, so far as we at present know, of the prostatic portion." He does not say whether he means organic or traumatic stricture. He is very accurate as regards the non-existence of prostatic stricture, but it is doubtful whether organic stricture occurs in the membranous urethra. There can be no question that many of the published descriptions of the seat of organic stricture are founded upon insufficient data, chiefly upon observation on the living subject only—a by no means reliable source of precision in such a matter. We insist, with Sir Henry Thompson, that the only method of conveying a correct idea respecting locality is to identify the contraction with the anatomical regions of the urethra, and not to trust to measurements from the orifice simply. He arrived at the conclusion from an examination of about three hundred museum preparations of stricture, that the part of the urethra most frequently strictured is the portion comprised in the inch anterior to the junction between the spongy and membranous portions, while behind the junction it probably never exists, except from some traumatic cause. Van Buren seems to be of a similar opinion, his chief authority, however, on the matter being Sir Henry Thompson.^a Mr. Fleming has seldom seen a stricture of the meatus "in which there was not an accompanying stricture in the region of the bulb." He does not say whether this opinion is founded upon *post mortem* examination or upon catheterism. For our part, notwithstanding his opinion, we believe the second stricture to be very unfrequent. If so common as he states it to be, it is curious that he should have had recourse to the female urethra to strengthen his position, for he gives the case of a lady who, with the smallest anterior orifice he had ever seen, had also a distinct obstruction at the neck of the bladder. As the lady ultimately recovered, his diagnosis remains unverified. Mr. Fleming describes "a valvular stricture of the urethra," a form of impediment which he was "not aware of having been before described." He calls this stricture a valvular flap, which projects across the course of the canal "laterally or antero-posteriorly," the free margin presenting in the direction of the orifice. As regards this stricture there is no necroscopic verifica-

^a A Practical Treatise on the Surgical Diseases of the Genito-Urinary Organs, including Syphilis. By W. H. Van Buren, A.M., M.D., and E. L. Keyes, A.M., M.D. New York. 1876.

tion mentioned, and we doubt that it was any other than the previously known crescentic stricture. At all events he has found catheterism more easy in cases in which he diagnosed the existence of this stricture, the penis being turgid rather than flaccid.

This work being, as we have already observed, addressed to students, the descriptions of instruments both practical and historical could scarcely have been too accurate, and yet we are constrained to remark that Mr. Fleming seems to have forgotten the history of the instrument he calls Holt's dilator. He suggests, as an improvement of his own upon this Holt's dilator, to have the instrument made an inch shorter. It is to be regretted that before he described this improvement he did not look into Perrève's "*Traité des Rétrécissements Organiques de l'Urètre*," where he would have learned that Perrève's dilator was so constructed. We have been favoured with an examination of Perrève's original instrument, the identical dilator from which the drawing in the *Traité* was made. It is, relatively speaking, a very short one.

We cannot proceed without animadverting upon the author's ungenerous silence regarding the exertions of many of our Dublin brethren in the surgery of stricture. We would not say one word upon the subject had he confined himself to a *resumé* of his "own experience," but having deviated from this course by allusion to a select few, we have felt it our duty to notice it. He is of opinion that "the ordinary method of gradual dilatation by means of bougies or catheters is preferable, in the great majority of cases, to any of the more formidable operations proposed for the cure of stricture. Very many persons become strangely reconciled to this mode of relief, tedious as it often is. It is true that in some instances the use of a simple bougie in the treatment of this affection is followed by symptoms of the gravest kind, but, as a rule, it is free from the risks which beset any of the bursting or cutting operations." Here we have a sweeping assertion as to the risks of these operations, without any figures to prove its accuracy. The relative value of these three modes of treating stricture being greatly debated at the present time, facts, and not mere statements, are required for its solution. We have not ourselves seen any bad results that, in strictness, could be attributed to one of them—divulsion—and when we consider that its enemies have been for a considerable period watching for opportunities to damage its reputation, it is marvellous how few cases of resulting mischief they

have been able to adduce against this operation, which may now be fairly called a cosmopolitan one.

We are, however, open to conviction regarding divulsion, but we must be met with well authenticated facts only. The question whether divulsion of a stricture with a dilator, or the slower method with the bougie or the catheter, is the best method of treatment being, he repeats, undecided, these are what surgeons require for its elucidation. Many observers believe that more mischief results from the aggregate of repeated introductions of the bougie or the catheter than from the single use of the dilator.

We are not prepared to assent to Mr. Fleming's explanation of the cause of the course of perineal abscess towards the anal region. We fancy that, according to his accurate description of the perineal fascia, the abscess, provided the fascia does not slough, should go in the opposite direction:—

“I have satisfied myself,” he observes, “that in such cases the course the fistula follows is anterior to the anterior layer of the triangular ligament, and posterior to the anterior division of the superficial fascia which identifies itself with the base of the ligament. These anatomical considerations will account for the fact that the progress of such abscesses is towards the anal region.”

Quite the contrary—they should pass forward, the identification of the base of the ligament with the superficial fascia preventing them from going backwards.

The following observations of Mr. Fleming, on *Paracentesis vesicæ*, we have no doubt will meet with general approbation:—“The demand” for this operation, he observes, “appears to him to be very rare indeed. . . . The case must be a very extreme one to demand it, and he is strongly of opinion that the only condition requiring it is laceration of the urethra from some form of violence.” Although we cannot accord an unqualified assent to the latter portion of his remarks, we believe that no matter how extensive the practice of any individual may be in the surgery of the urinary organs, the operation should be very seldom required. Mr. Fleming, who, in a “very extended practice,” had occasion to perform the operation but twice, has, on this question, been corroborated by the practice of Sir Henry Thompson, who has performed it but six times—twice for prostatic enlargement, and four times for retention from stricture.

Case LXVII., one of sacculated bladder, is most instructive, but

would have been still more so had the author described the *post mortem* appearances observed in the urethra, and the condition of the prostate. It was diagnosed during life that the prostate was not enlarged, and that because a No. 10 catheter could be introduced there was no stricture. Curious to say, the *post mortem* examination revealed a dilated left ureter and a contracted bladder, at the back of which there was a pouch larger than the bladder, the pouch containing half a pint of fluid.

Chapter VIII. is devoted to some diseases of the testicle, scrotum, and the round ligament in the female. The cases will repay perusal. Case LXVIII., for example, was one of tunica vaginalis hydrocele that simulated inguinal hernia. It was of the hour-glass form and entered the inguinal canal. Mr. Fleming considered it "to be a form of hydrocele to which he had on previous occasions drawn attention."

Cases of this description did not escape the sagacious eye of Sir Astley Cooper who, upwards of seventy years ago, drew attention to them in the following words:—

"I have seen cases of hydrocele, however, in which there was unusual difficulty in deciding upon the nature of the complaint. When it became so large as to extend upwards through the abdominal ring to the abdomen, the form of the tumour is precisely the same as that of hernia, and it even dilates when the patient coughs, owing to the sudden pressure upon that part of it which lies above the ring."

The author's heading to Case LXIX. will, we fear, appear misleading, for what he calls "a large double encysted hydrocele of the testis," turned out to be one of encysted hydrocele of the cord, as he mentions in its description.

Mr. Fleming justly takes some pride for his account of granular swelling of the testicle in the infant—a condition, he informs us, unknown to Mr. Curling, until a specimen was shown by him to Mr. Curling on the occasion of one of his visits to Ireland. He refers the reader to Mr. Curling's work "for the history of the symptoms detailed, and for the treatment to be adopted"! This certainly is complimentary to Mr. Curling, but more or less impairs the usefulness of this work.

Case LXXIV. is an example of the severe local and general symptoms that sometimes supervene upon operations in children. It was a case of cystic tumour of the scrotum—a disease, the proper treatment of which the author is "not fully convinced of." He

has performed subcutaneous section, and has tapped and injected them. "The result of each has been uncertain," and he has seen "one excised by one of our ablest surgeons with almost fatal results."

We had better here draw the reader's attention to a printer's mistake in the reference to the illustration of the ulceration Mr. Fleming has seen in wine bottlers. The figure should be 4 and not 3, as stated in the text.

We have now arrived at Mr. Fleming's observations on stone in the bladder and kidney, and have again to refer to another of his statements that he leaves uncorroborated by facts. In speaking of the frequency of stone in the bladder in Ireland, he observes:—"I believe I should exaggerate if I stated that the average number of cases of stone operated upon *annually* throughout Ireland by lithotomy or by lithotritry, in our various hospitals or infirmaries, has exceeded twelve ever since the period alluded to." He is speaking of Mr. Dease's experience of a century ago—this distinguished surgeon having stated that "the aggregate number of such cases in the course of *te* years in all the hospitals then in Dublin, some of which cases were from the country, was only twenty-eight." We question whether Dease's statement can be considered conclusive. If we call to mind the great improvement that more recent years have witnessed in the detection of stone, we suspect that the stone was frequently overlooked. At all events Mr. Fleming has probably understated the frequency of stone in this country. This suspicion is almost corroborated by an observation of Mr. Fleming himself. After stating his belief that the annual number of cases of stone in Ireland averaged but *twelve*, he informs us that "not a few of these cases have, of late years, fallen under his care whilst surgeon to the Richmond Hospital, and have been the subjects; some of lithotritry, others of lithotomy." Surely it cannot be possible that for the number of years he was surgeon to the Richmond Hospital he was almost the only surgeon of all Ireland who was operating for stone; for, recollect, he fixes the cases for all Ireland at twelve annually, not a few of which fell under his care. Not a few, of twelve, must considerably reduce the number. It behoves the hospital and infirmary surgeons of Ireland to give us some information on this matter. We should be greatly astonished, indeed, were the clinical records of all Ireland unable to disclose more than twelve cases of stone annually since the time of Dease!

In Case LXXVI., a lithotrity case, Mr. Fleming injected the bladder before crushing, used Weiss's fenestrated lithotrite, and seems also to have had recourse to the steel evacuating catheter. At a crushing in three days afterwards a smaller fenestrated lithotrite was used, and the evacuating catheter was repeated. The scoop lithotrite alone was used at the third sitting, and at the fourth sitting he failed to work a manual-pressure lithotrite, and had again to use the scoop lithotrite. In this case, notwithstanding the assistance of Sir Philip Crampton and his ingeniously-constructed glass-exhausting apparatus, a fragment remained in the bladder, which, in a couple of months after the cure was considered complete, entered the urethra and caused a "frightful hæmorrhage." The man, however, made a good recovery.

Mr. Fleming winds up his description of case LXXVII.—a case of lithotrity in a child—with the following statement:—"My impression respecting lithotrity in boys is so unfavourable that I have not since adopted it." We have seldom seen it performed, but one case is fresh in our memory in which the most fearful torture resulted from the operation on a boy by one of the most skilful lithotomists of his day, and one who had not a few of the Irish twelve annual lithotomy cases of which Mr. Fleming makes mention.

Case LXXVIII.—Lithotrity. The patient was only twenty years of age. The bladder was injected and the fenestrated screw lithotrite used. After the sitting, he was allowed to micturate in the erect posture. A preliminary injection is not mentioned at the second sitting, nor at the fourth, when a flat-bladed lithotrite was used, the patient being under the influence of chloroform. On this occasion also he was given directions "not to pass water whilst recumbent." There is no mention of the third sitting.

Case LXXIX.—Lithotrity. The patient was aged between sixty-five and seventy. The case may be used in an argument against the preliminary injection of the bladder in lithotrity; for, Mr. Fleming having introduced a fenestrated lithotrite to measure the stone, which he seized and could not dislodge, he at once crushed it. The bladder bore the several crushings well; the subsequent irritation was slight and easily controlled by mild anodyne treatment. There were several subsequent sittings, and, at last, a cure seemed to follow upon the removal of a particle of stone with Lühr's Civiale's scoop lithotrite. We were glad to find Mr. Fleming

admit that "it is possible" that the cure of these cases "might have been more quickly effected, and with perhaps less of suffering, under the present advantages of more modern lithotritry. Its several stages are now much simplified—there is no necessity for a special bed; no preliminary injection of the bladder."

The author pays a deserved tribute to the improved lithotrite of Weiss and Sir Henry Thompson, but he gives these gentlemen more credit for their inventive genius than they themselves claim. He has apparently forgotten that it is to Civiale and Charrière, as Sir Henry Thompson shows, we are indebted for one of the greatest of lithotrite improvements: the power of converting the sliding into the screw movement, and *vice versâ*. But, although we owe so much to Civiale for the principle, the mode of carrying it out is far more simple in the Weiss than in his own lithotrite. So true is this that Civiale, before his death, substituted Weiss' and Thompson's for his own lithotrite in his own operations.

Mr. Fleming's practice differs from the matured practice of Sir Henry Thompson on a most important point—the lithotrite to be used. Mr. Fleming writes thus:—"In the selection of the lithotrite, I am guided by the calibre of the urethra, always providing that the sliding portion of the shaft of the instrument shall have free space for loose movements within the canal, and I select either the lithotrite fenestrated or with entire blades, according to circumstances . . . but, if at a first 'sitting,' and for the purpose of breaking a stone, the lithotrite must be fenestrated."

Now, what has the vast lithotritry experience of Sir Henry Thompson taught him regarding the safety of the fenestrated lithotrite:—"Usually," he observes, "when the stone is large and hard, it has been the practice to begin with the fenestrated instrument—that is, one in which the female blade is entirely perforated, allowing the male blade to pass through it. This mode of construction renders the instrument always more or less dangerous, since its edges must meet and fit accurately; the teeth must be sharp; the blades require much space in the bladder, in order to include a stone between them, on account of their width; and the fragments produced by their action are necessarily hard, sharp, and angular. Such an instrument should be used as little as possible. I never use it now, although I did so many years ago. Certainly for eight years I have not employed one." The observations of Sir Henry Thompson refer to the lithotrite as manufactured by men of great constructive capacity, like the Messrs. Weiss,

for example. As made by them the slender non-fenestrated lithotrite is of extreme tenacity and power and may be used with perfect confidence and safety.

Mr. Fleming refers the reader for directions for seizing and crushing the stone to the fourth volume of Holmes's System of Surgery. The absence of these directions lessens the usefulness of this portion of the work, at least as regards the student.

The remarks respecting the detritus, in the concluding observations on the lithotritry cases, are thoroughly practical and to the point:—"As regards the detritus, it was, it may be said, allowed to escape by the natural efforts of the bladder, and no very serious consequences ensued. Indeed, respecting this stage of the operation, I am of opinion that over-minute pulverisation of any portion of stone has its disadvantages, and I must acknowledge that I am much opposed to the forcible traction through the urethra, although advocated by high authority." This is as it should be, and may have some influence in checking the indiscriminate churning of detritus in the bladder with Clover's apparatus. We say indiscriminate, because cases do occur in which it is of much service.

As we proceed we find another reference for details of operative proceedings. Thus, in speaking of lithotomy, Mr. Fleming tells the reader to consult systematic works for them, which is rather hard upon those who have invested their money in this book under the impression that it contained them.

Mr. Fleming gives an interesting series of lithotomy cases, nearly all of which were successful, but the majority were young subjects, in whom the mortality from lithotomy is very low. In one of the fatal cases he is so disinterested as to admit that after the bladder had been opened he was unable to catch the stone. The boy was subsequently seized with scarlatina, and died. A *post mortem* followed, and an almond-shaped stone was found impacted at the neck of the bladder.

He concludes the lithotomy cases with the details of a case in which he removed a stone from the female bladder by dilating the urethra with a sponge tent, and then incising the canal by means of Crampton's cutting dilator. The patient has since remained perfectly well. The stone was small, and, we fancy, might have been easily crushed.

Mr. Fleming here gives us some instructive observations on the pathology of the urine in connexion with lithotritry and lithotomy, and its bearing on the form of operation to be selected.

Speaking of the relation of pus to albumen, he observes: "The principle is supported, to which I attach some value, namely, that pus in the urine, no matter in what amount, is to be traced to other sources than the kidney, or, in other words, that it is added to the urine *in transitu*, after it has passed from the kidney, if the amount of albumen in the supernatant fluid does not bear a proportion to the pus in the deposit." Through an oversight he does not say what this proportion should be.

Mr. Fleming makes a few remarks respecting the "selection of operation" which, as far as they go, are valuable; and he alludes to those cases of extremely small stone that have been possibly carried out of the bladder with the first gush of urine from the wound, leaving the operator in doubt for the moment as to accuracy of diagnosis.

The observations on chloroform in lithotomy are judicious, and particular attention is drawn to "violent forcing and straining efforts, as if the whole contents of the pelvis were about to be protruded," while the patient is under the influence of the chloroform, as well as to a rolling of the pelvis, which renders it almost impossible to fix it. "The remedy for those anomalous effects of chloroform is, in the opinion of some, to press this agent, and to overcome them. This expedient is, however, not successful in many instances."

Speaking of sounding for stone, we are warned to be "prepared for sounds and sensations which may be due to such causes as hypertrophied walls with phosphatic deposits upon their rugous lining membrane, or an encysted tumour, or a rapidly contracting bladder or pouch." We imagine there are few surgeons in the habit of using the sound who have not felt a "rapidly contracting bladder" strike the end of the sound. It is a most deceptive sensation, and the author has done well to caution the student regarding it. Mr. Fleming, however, talks of a rapidly contracting "pouch," but, as these bladder pouches are uncovered by and devoid of muscular structure—at least some excellent pathologists say so—we cannot understand how they could contract, at least in the manner he mentions.

Of the non-detection of a stone by one surgeon, which another at once feels, Mr. Fleming speaks thus:—

"Surgeons are sometimes disappointed when they learn that a case, in which they have made careful but unsuccessful examinations, has at once satisfied the suspicions of another surgeon at a distance, whom the patient has consulted. This has happened to myself. I have failed to detect a

stone afterwards struck in London; and I have found stones which London surgeons failed in discovering. There should be no reflection upon the skill of the surgeon in such cases. The explanation is, that the stone has been encysted or temporarily fixed in a portion of the bladder, and has not obeyed the movements of the body by the surgeon. But a long railway or car or sea journey is often attended by a sudden loosening of the calculus, and it is then detected by the person who may next be consulted on the occurrence of painful symptoms. This suggests the expedient in doubtful cases of applying the test of rough exercise before finally deciding."

The suggestion is worthy of remembrance, but it must be conceded that without even the rough exercise a stone will give the slip to one surgeon which another will readily discover. It is not very long ago we had unequivocal information regarding a case in which one of the most distinguished surgeons of our time—one of world-wide fame—failed in finding a stone which, on the same day, was struck by a surgeon in an adjoining street. The distance between their houses was short, but, no doubt, may have been sufficient to allow of the shifting of the calculus. Even without the disturbing element of a journey, surgeons must differ greatly in the tactile capability for detecting stone.

Mr. Fleming insists upon a rigid adherence to the time-honoured precept that "the surgeon should always feel the stone himself before commencing the operation." Owing to a neglect of this rule we have seen a person cut for stone whose bladder did not contain one.

In lithotomy he is in favour of leaving the staff to the care of an assistant. This is our own practice, and, although it has its disadvantages, much can be said in support of it.

He gives some drawings of a sound similar in principle to the sound of L'Estrange. It seems well adapted for sounding, for measuring, and, in some cases, for removing a stone.

Allusion is made to a second lithotomy in the same subject as follows:—"I am not aware of more than one case in this city in which the operation of lithotomy was had recourse to a second time for a recurrence of vesical calculus." He does not mention if this took place recently or not; and, as we have assisted in one such case, his reticence of dates is tantalising. In our case, however, the stone was much larger than an almond, so that it may be safely concluded it was not identical with the one to which he refers.

Plate XIV. contains some excellent and effectively arranged drawings of urinary calculi, and of portions of a fibro-calcareous

tumour of the uterus. Figs. 1 to 7 represent calculi that were passed on the same occasion by a gentleman; they strikingly illustrate the good luck of some individuals.

Case XCIII. was a most melancholy one. It occurred in a woman, the subject of an enormous fibro-osseous tumour that apparently sprang from the anterior wall of the uterus, and ulcerated into the bladder, causing many of the rational signs of stone. Under the supposition that it was such, Mr. Fleming proceeded to extract it, trying first dilatation of the urethra. "Failing to remove the foreign body with a forceps, he introduced a lithotrite, and broke some portions of it, and then extracted about two ounces of calcareous fragments; but finding an enormous quantity still remaining in the bladder, it was deemed best to have recourse to the supra-pubic operation, and he removed thereby some large masses. The quantity remaining being still very great and adherent, he desisted from further interference, and sent the poor woman to bed." Wonderful to relate, the author tells us, "she had great relief from the operation, but she sank from exhaustion in four days." The particulars of the analyses of the concretion by Professor Bennett, W. Arthur H. Hassall, and by T. W. Grimshaw, are mentioned, but we must refer the reader to the work itself for these.

We found some difficulty in penning our remarks on Case XCIV. In this case, we are told by the author, that he detected in the bladder "the sharp click, or ring, of a stone;" and that he "felt a resistance as of a calculus encysted in the walls of the bladder." Some of his colleagues, however, "were not satisfied of the presence of the foreign body." Nevertheless he performed Allarton's operation; but when he got into the bladder he "felt no stone," nor was he "able to detect any detritus or roughness on its walls." We must here observe that he is not very disingenuous as regards those of his colleagues whose tactile sense, evidently accurate, failed to discover a stone, for he goes on to say that during the operation, before he "introduced his finger, a large gush of urine came away, and some of the calculi which he felt before he operated may have escaped." Finally, he assumes that the "calculi occasionally passed along the ureter to the bladder, and were detected there, ultimately escaping by the urethra." The right kidney was beset with calculi. The author, with much *naiveté*, attributes the failure of his colleagues to feel a stone to the possibility of the foreign body at the time of sounding

being so invested by a thick coating of mucus as to muffle the click, so that according to this explanation, whenever his colleagues sounded, the calculus mysteriously enshrouded itself with mucus, but, when he did so, it joyfully emerged from its covering, and good-naturedly exposed itself to the beak of his sound. In justice, however, to the memory of his colleagues, we cannot let the opportunity pass without observing that had he been guided by the negative result of their sounding, he would have been spared the chagrin that must have followed upon this phantom lithotomy.

Plate XIII. illustrates a very instructive case of the late Professor Smith's, which corresponded to a great extent with Case XCV. of the author. In Mr. Smith's case the bladder contained an immense number of small calculi, and several studded the urethral tract. Cases of this kind are not altogether unique. For example, a very instructive one has been recorded in the 3rd volume of the *Philosophical Transactions*, the subject of the case having been a boy from whose bladder 96 calculi were taken.

Chapter X. contains some useful hints on phimosis, as well as a drawing of a quadruple hook, or four-pronged hook for enabling the surgeon to divide the "integument and the mucous membrane on the same level." "The section," he tells us, "frequently turns out so satisfactorily that the divided surfaces lie in apposition, the mucous membrane and the skin being accurately adapted to each other." For the mode of using the instrument the reader should consult Dr. Fleming's work, in which full details for doing so are given. This instrument, which resembles on a small scale the grappling-iron used in the search for dead bodies, is an old acquaintance of ours, having been described by Mr. Fleming in the *Dublin Hospital Gazette* for May, 1857. About the same period we read in a French journal an account of an instrument constructed on the same principle, but as we have no note of either the name of the journal or describer of the instrument, we must leave Mr. Fleming in possession of the honour of its invention.

The concluding chapter is devoted to morbid conditions of the urine in children—a most important subject, and one deserving of the utmost attention on the part of the student. The generality of surgeons will probably agree with Mr. Fleming that colourless crystals of lithic acid are more frequently seen in the urine of the child than in that of the adult, and there can scarcely be a doubt that bladder symptoms dependent upon an abnormal condition of the urine, leading to the suspicion of the presence of stone, are not

uncommon in the young subject. "Two practical lessons," he observes, "should not be lost sight of," when this morbid condition of urine produces "local or general irritation"—viz., "to alter as quickly as possible this morbid condition of urine, and to suspect the presence of calculus should it be obstinate. We regret he has not further elucidated the following point:—"In the ordinary diseases of children, acute and chronic, blood, mucus, pus, or epithelium," are constantly found in the urine. Epithelium and mucus are common, no doubt, but we have not ourselves so frequently seen the other deposits in the "ordinary" diseases of children. The following caution cannot be too frequently impressed upon the student:—

"When pus is persistent and obstinate, local inspection must be made in the female child, as morbid secretions from the vulva and vagina are not uncommon in such children if badly cared, and the urine *in transitu* will be loaded with pus cells, mucus, and even with an amount of phosphates sufficient to render it neutral or alkaline, and this quite apart from urinary disease, although attended with much urinary irritation."

There is a source of irritability of the bladder in girls which Mr. Fleming has occasionally encountered, and with which it is important to be acquainted. On this point he speaks as follows:—

"Irritability of the bladder in girls may arise from another source than that of general engagement of the vaginal mucous membrane in the secretion of pus. I allude to an ulcerated fissure of the vagina, resembling a similar affection engaging the rectum in the adult. The agony attending this can hardly be exaggerated: the principal suffering is referred to the bladder, and the pain during and after micturition is most acute. It is also quite intelligible that the whole mucous membrane of the vagina, in consequence of this partial ulceration, may become secondarily engaged, and that the purulent secretion will be proportionably profuse."

Dysuria in female children, particularly when teething, has been specially mentioned by writers on the diseases of the young subject. In some of the cases the dysuria preceded the muco-purulent discharge from the vulva and vagina. These writers, moreover, describe the itching and smarting of the parts from which the children likewise suffer. Whether the smarting is to be attributed to mere excoriation, or to the fissure described by Mr. Fleming, we cannot pretend to say. He, however, deserves some credit for having drawn special attention to these fissures, which might be unwittingly attributed to laceration from violence. According to

Mr. Fleming's experience, purulent urine in children occurs more frequently in boys than in girls, and he gives a couple of instructive cases to illustrate his observation. One of these strikingly resembled a stone case, the rational signs of stone being "very prominent." The bladder, however, "was examined with a view to the presence of calculus, but none was detected." But the case is not conclusive, no further particulars being given regarding it. We are inclined, however, to admit that in children with stone purulent urine is rare. He lays much stress on the "uniform miscibility of blood or pus with urine whilst passing from the urethra," as an "invaluable guide in estimating their source." He is rather vague in his observations thereon, but we infer his meaning to be that, when the urine is purulent throughout "the whole act of micturition, there is probably renal disease." He observes that pathology "teaches us how deceptive symptoms are as to the existence or site of organic lesion of the urinary apparatus even in the adult." In a case of irritability of the bladder in the adult, he has "found the kidneys extensively diseased and the bladder healthy, apparently, although the latter was *nominally* the seat of all the suffering of the subject of it." We have seen many cases of this description, and, if we do not mistake, Sir Benjamin Brodie, years ago, cautioned us against being misled by such. Mr. Fleming favours the use of creasote in the irritability of bladder "attendant on the ordinary worms of children." He constantly directs it "with or without calomel, according to circumstances." Of another cause of irritability of bladder he writes thus:—"In many cases he will find that the orifice of the prepuce is at a considerable distance from the extreme end of the glans; that that orifice is so small as to admit with difficulty the end of an ordinary probe; and that when the child makes water, the urine does not escape in a uniform stream; that it is scattered, and that during its transit the prepuce is distended into a pouch by the accumulation of the urine within it, the glans being at a considerable distance behind. Such a state of organ will not necessarily entail symptoms of irritability of bladder." Here the author becomes obscure, for he goes on to say that the "contrary, as in the case of the female child already noted, may be the fact," and leaves us disposed to ask whether this female child had a penis, the glans being "at a considerable distance behind the narrowed preputial orifice?" Be this as it may, he proceeds to remark "that when deranged action or function of the urinary system arises, no matter from what cause,

the presence of it will be found to be often attributable to the lesion" he has mentioned—the narrowed preputial orifice. Surely he does not mean that if it arises from "no matter what cause," it could be solely attributed to the narrowed orifice.

We have now almost brought our labours to a conclusion, and must express our regret that there is much evidence through the work to render it exceedingly probable that Dr. Thomson's hands were more or less fettered in his editorial duties. For we cannot for a moment suppose that it would have contained so many oversights had a gentleman of his well-known literary and other accomplishments been altogether unhampered. These oversights, however, can scarcely be said to militate against the great practical value of the work, almost every page of which contains instructive and useful lessons.

Finally, we cannot conclude without saying a few commendatory words in favour of its excellent typography, and of the great taste displayed by the Messrs. Fannin in its production. In these respects it will bear comparison with the most perfect issues from any press.

The Cure of Rupture, Reducible and Irreducible, also of Varicocele and Hydrocele, by New Methods. Pp. 196. By GEORGE HEATON, M.D. Boston: H. O. Houghton & Co.

THE title of this book is striking enough to stay the surgeon's attention. "Cures" of ruptures are so rare that any one who succeeded in establishing even a tolerably certain method for the radical treatment of the affection, would earn for himself an imperishable fame. Such a pretension Dr. Heaton makes, and he backs up his declarations with an array of cases which at once excite our wonder.

Starting with the principle that herniæ are due to the weakness of the fibrous structures involved in the containing parts, Dr. Heaton has devoted his attention to strengthening these. In securing this result, "any inflammation, except of the mildest grade, must be carefully avoided." It is owing to the existence of this, in excess, that Wutzer's, and Wood's, and his own earlier procedures failed, because nature treats the presence of the inflammatory products as foreign bodies, and gets rid of these tissues as speedily as possible. Eight years devoted to experiment resulted in the

establishment of a process which the author calls the “method of tendinous irritation.” The surgeon provides himself with an ordinary subcutaneous syringe, and—

“Armed with the instrument, which is supposed to be charged and prepared, the operator introduces its beak into the inguinal canal, but outside of the sac, if this has been suffered to remain, in the following manner: invaginate the right forefinger in the scrotum and find the external abdominal ring, then with the left forefinger press perpendicularly upon the integument directly over this ring, and use sufficient force to, if possible, press the integument together with the finger directly into the ring. The left forefinger being at or in the ring, the spermatic cord and the sac, if in the way, are to be pushed to one side so that nothing may remain between the external pillar of the ring and the finger except the integument and subjacent superficial fasciæ. Keeping the left forefinger thus, take the instrument in the right hand and introduce its freshly sharpened and polished beak *quickly*, penetrating the integument and superficial fasciæ, just passing but not grazing the external pillar, and entering the canal at once. Then remove the left forefinger and gently insinuate the beak further on, well into the canal, exercising the greatest care not to impinge upon the spermatic cord, which is sensitive to the slightest touch, or upon the fibrous walls of the canal. To wound any of these parts endangers the success of the operation, and to penetrate the transversalis fascia would be particularly unfortunate. If the operator in attempting to pass through the ring should impinge upon or transfix one of the pillars (an accident to which the tyro is very liable), the instrument will not be able to be freely and easily moved about, which it is to a remarkable extent when the canal is successfully entered. But before proceeding any further the surgeon may do well to confirm his diagnosis of position by transferring the instrument to the left hand, while with the right forefinger invaginated in the scrotal tissues he explores the inguinal region, and examines the exact situation of the beak. Beyond the prick of the puncture the patient suffers but little pain if the introduction is skilfully performed. But any awkward movements of the beak about the spermatic cord will cause sharp pain, which is referred to the testicle or to the deeper parts of the abdomen.

“Having satisfied himself that the beak of his instrument is in the canal, the surgeon then deposits about ten minims of the liquid irritant, emitting it drop by drop, and spreading it as much as possible. The beak of the instrument should be well swept about while delivering its contents, passing around the exterior of the sac if unreduced and wetting all the fibrous tissues. Particular care should be taken that the intercolumnar or arciform fibres and the inner edges of the external ring are wet with

the irritant. The canal is usually found much more free than would be anticipated, and any adventitious adhesions can be either broken or avoided. A small though essential amount of the irritant should be placed in the extreme upper portion of the canal, so as to operate upon the fibres embracing the internal abdominal ring. Owing not only to its proximity to the abdomen, but also, and more especially, to the usual presence in the upper part of the canal of a few muscular fibres of the internal oblique, the sensitiveness to irritation here is extreme, and the slightest amount of material produces all the effect that is usually desirable.

“Having wet the entire fibrous interior of the canal and of the inguinal rings, the beak is then withdrawn quickly so that none of the injection may be left in the cellular tissue and fasciæ lying beneath the integument and just exterior to the external abdominal ring. At the instant of withdrawing the beak press the finger over the puncture, thus preventing any oozing of blood which might occur if the skin is delicate, and also in the case of a hernia with a free opening hindering any of the injection which has not been absorbed from oozing outwards. The application of the irritant may cause some slight immediate pain, which is soon allayed by the morphine which is contained in the injection. The previous protrusion should not be allowed to descend after the application of the irritant, nor the patient be permitted to assume even the sitting position, until a suitable bandage or other means of support has been properly applied.”

The irritant is composed of “Thayer’s fluid extract of quercus alba, prepared in vacuo, one half ounce, of the solid alcoholic extract of quercus alba about 14 grains,” with sulphate of morphine added in the proportion of a grain to the ounce. A special bandage is applied, and the parts are kept at rest. Sometimes the support may be removed in about six or eight weeks, but in others it must be retained for a long period.

The cure of varicocele consists in applying silver wire at two or three places in the course of the vein, but there is nothing “new” in this. Neither is there any novelty in the use of red precipitate for the cure of hydrocele after the fluid has been removed. At all events it has been long practised in this country, although it may not have yet reached our brethren across the Atlantic. There are many details which the surgeon whose curiosity is excited will find in Dr. Heaton’s book, which we have not space to refer to further. We can only recommend it as one of the newest and most interesting contributions to a very difficult subject.

Clinique Médicale de la Pitié. Par T. GALLARD, Médecin de l'Hôpital de la Pitié, &c. Paris: Baillière et Fils. Pp. 635.

THE twenty lectures which make up this work embrace a wide range of subjects. Alcoholic gastritis, ulcer of the stomach, typhoid fever, writers' cramp, arterio-venous aneurism, abscess of the liver, vaccine, &c., are some of the topics discussed. We think it would have been better had the author confined himself to a narrower scheme, and entered more fully into details. The chapter on aphasia, having in view the advances made by Broca, Brown-Séguard, and Charcot, is particularly disappointing; nor will any one, save a junior student, gain much information from the twenty pages in which such a subject as typhoid fever is examined and dismissed. We believe the chapters on diseases peculiar to trades will be found not only the most original but also the most useful part of the work. Writers' cramp is described at length as well as the various mechanical devices of Velpeau and others to obviate it, and the effects of sulphide of carbon on caoutchouc workers are clearly and graphically indicated. Mercurial erythema, as the older English writers termed it, is stated to be merely simple eczema, differing from other forms of eczema only by the ease with which a cure may be effected. Curiously enough among the evil consequences of mercury erethismus is omitted. For the manner in which the work is printed and published we have nothing but praise.

Curvatures of the Spine; their Causes and Treatment. By RICHARD BARWELL, F.R.C.S. Third Edition. London: Macmillan & Co. 1877. 8vo. Pp. 238.

THIS work, the third edition of which has recently been published, is one which deserves the attention of all surgeons who devote themselves especially to this particular branch of surgery, and the fact that it has reached a third edition is sufficient proof that the labour which the author has devoted to it has not been labour in vain. The motive with which he undertook this work is the only proper one with which a man ought to undertake any publication on medicine or surgery—namely, to supply some real deficiency in these sciences, and to add something new and useful to our knowledge of them. He says:—"Having failed to find in books a satisfactory theory of those conditions which produce lateral curva-

ture . . . It then appeared that, to carry out my design, I must work for myself."

With this object he commenced a series of experiments and observations, with a view to detecting the forces by which the various distortions of the spinal column might be caused; having satisfied himself as to the muscles at fault, either through their over-activity or want of power, he then endeavoured to correct the deformities rather by restoring the equilibrium of the opposing muscles than by merely supporting the spine itself. The volume contains reports of a considerable number of cases in which most satisfactory results were obtained, and illustrations are given of the bandages and appliances with which they were treated.

We hope that the experience of orthopædic surgeons may justify the high opinion which Mr. Barwell expresses of his own work, in the preface to the third edition, which, he says, "has been revised and slightly modified; certain forms of bandage not previously depicted have been added, and certain phases of distortion have been more fully described. The delineation, therefore, of lateral curvature, its origin and treatment, is complete in itself, nor do I think that anything of importance has been omitted."

We can hardly hope that there is nothing of importance yet to be learned in the treatment of spinal curvature beyond what is contained in the book now before us; but, as there is some original matter in it, we recommend it to the notice of those who are engaged in this branch of surgery.

TREATMENT OF STRYCHNIA-POISONING.

A CASE in which a man took "several grains," "over two," of strychnia with suicidal intention, is reported in the *Philadelphia Med. Times*, Oct. 13th. There were general convulsions with intervals of relaxation. The treatment successfully adopted was, after giving an emetic, the administration of the vapour of ether, and bromide of potassium in large doses by the rectum and mouth. With the two first injections, at hour intervals, of an ℥iiss. and ℥i. respectively of the bromide, gr. lxxx. and gr. xc. of chloral hydrate were also given; and in another hour ℥ii. of the bromide dose were injected. The reporter, Dr. Sandes, remarks that nothing could be more happy than the effects of ether in this case, but he is disposed to give it only a secondary place as a curative agent. It relaxed the spasm sufficiently to allow of the administration of the remedies, but the bromide, as recommended by Prof. H. C. Wood, was evidently the true physiological remedy. Recovery was prompt.

PART III.

HALF-YEARLY REPORTS.

REPORT ON SURGERY.

By WILLIAM THOMSON, A.B., M.D.; Fellow and Member of the Surgical Court of Examiners, Royal College of Surgeons, Ireland; Surgeon to the Richmond Hospital, Dublin, &c.

PALATO-PLASTY.

UNDER this title, Dr. David Prince, of Jacksonville, Illinois, publishes a paper in which he makes several suggestions regarding the operative measures adopted for the cure of cleft palate. Upon the division of muscles he observes:—

“The division of the palato-pharyngeus and of the palato-glossus by Fergusson, was adopted on the supposition that these muscles tend to pull the two halves of the palate away from each other, but this supposition is believed to be erroneous. It is found by observation of the movements of the curtain of the palate in halves that the contraction of both these pairs of muscles approximates the opposite sides of the cleft, causing the two halves of the uvula, in most cases, to come in contact.

“The mode of this will readily be understood by recalling the manner in which the tongue is protruded by the genio-hyo-glossus. So, while the palate is stiffened by the fibres of the tensor palati, the levator palati, the azygos uvulæ, and by a few fibres of the palato-glossus and palato-pharyngeus, the main portions of these last two muscles engage in approximating the two halves. To cut these muscles is, therefore, physiologically absurd, besides endangering their future functional perfection. Without the action of the palato-pharyngeus, the perfect articulation of the gutturals is impossible. The loss of the function of the palato-glossus would not be seriously felt in articulation.

“If this reasoning is founded on correct observation, the division of these muscles must speedily go into disuse. Their division is the result of the fear of the hæmorrhage arising from the vertical incisions first made by Dieffenbach. Now that we have a safe way of making these incisions, we may dismiss this fear.

“The muscular fibres of the posterior pillars are chiefly those which

approximate the two sides of the palate, at the same time that the palate is drawn backward by the superior constrictor to the posterior wall of the pharynx, by which movements communication is cut off between the lower pharynx and the upper. In this act the two sides of the pharynx are approximated; and if the palate were divided vertically in the centre and again united by sutures, these muscular fibres would act with the sutures and not against them.

“Again, the muscular fibres of the anterior pillars are those which approximate the two sides of the pillars at the same time that the palate is drawn forward to prevent communication between the mouth and pharynx. If, as before, we suppose the palate to be divided vertically in the centre and again brought together by sutures, the action of these muscles would be *with* the sutures and *not against* them.

“Again, it is very important to the future completeness of the function of the palate that these four muscles should have their perfect activity. If the palato-pharyngeus is impaired in its action, the communication between the upper and the lower pharynx can never be closed. The impaired action of the muscular fibres of the anterior pillars is less important in relation to the voice, but the action is essential to some of the functions of the mouth, among which is the use of the blow-pipe. The division of these fibres, either by incision or by the heated wire, is to incur the certainty of the impairment of their power of contraction, and the risk of its complete and permanent destruction.

“The objection here urged does not apply to the division of those muscular fibres which traverse the veil of the palate transversely. Their action is directly antagonistic to that of the sutures, and hence the tendency is to pull apart the approximated halves of the palate. In this view there should be a vertical incision on each side, dividing the transverse fibres of the circumflexus or tensor palati; and the more complete this division, the more complete is the suspension of the traction upon the sutures, to remove which is the object of the incision.”

The objection to this free vertical incision has been the liability to severe hæmorrhage, but this may be obviated by the use of the galvano-cautery. The methods of applying this means are explained, and illustrated with numerous woodcuts. For paring the edges of the fissure, a number of ingenious scissors, made by Leslie, of St. Louis, are figured.

With reference to the application of sutures, he makes the following suggestion, which seems to be valuable:—

“The wire which is to pass through the cautery openings, and is to encircle the central part of the plate, is first armed with an oiled paste-board square, having a small perforation in the centre; and as the wire

is drawn through, the other end of the wire is armed in the same way. As the wire is drawn down to be twisted, these paste-board squares are made to slide into the fissures occasioned by the cautery, so as to keep the wire from cutting into the tissues. By this means a very considerable degree of compressure upon the inner cauterised surfaces may be secured, without the subsequent ulcerating of the silver wire into the tissues. All possible strain upon the suture line is thus avoided. If union fails it must be from some other cause than the mechanical separation of the surfaces. It is also convenient to place a square under the central part of the wire last mentioned, in order to cover the points of the wires constituting the principal stitches. The tongue is thus protected from contact with their points.

“In the preparation of these squares or wafers, thin paste-board is cut into squares, one-third to half an inch in diameter, and perforated in the centre. The wire passes through this perforation, and the squares are slipped down at the time of tightening up the wire, so as to bring the paste-board between the wire and the tissue. In order to prevent the early formation of an infectious compound in the paper by the decomposition of the mucus and saliva, the squares are first soaked in a solution of carbolic acid in castor oil, one in eight. By the time these agents have been dissolved out of the paste-board, the cut surfaces have become proof against the absorption of septic products.

“The employment of a spray of ether during this and other operations upon the mouth and fauces is of no little importance. The patient is first etherised in the ordinary way. The gag is then placed between the jaws, and an assistant commences to blow a spray of ether into the patient’s mouth. This he does constantly, except when interrupted by such of the procedures as are inconsistent with it. Care is taken not to blow so continuously upon one spot as to freeze it.

“The flow of blood from the surfaces is very much diminished, and much time is saved which would otherwise be consumed in applying the etherised napkin to the patient’s mouth.”

THE TREATMENT OF CHRONIC PHARYNGITIS.

Dr. Foulis (*Glasgow Medical Journal*, Oct., 1877), having tried in vain the usual remedies for chronic pharyngitis in a large number of cases, resorted to the plan recommended by Carl Michel (*Deutsche Zeit. für Chirurgie*, 1873)—namely, the application of the actual cautery. Michel uses the galvano-cautery, but this being expensive, Foulis had some ordinary irons made. These were heated in the fire and applied. “The effect was quite what could be wished. Not only were the thickened patches easily and completely destroyed by the cautery, but its use was attended with but trifling pain, and

there was absolutely none of the former hawking and expectoration of mucus which the soda-lime caustic occasioned. A little swelling often followed the use of the hot iron, reminding the patient, for twenty-four hours or so, of what had been done, but I have never seen any more serious disturbance after it. This feeling of swelling and dryness was easily combated—a little bit of fresh butter flavoured with lime-juice, allowed to melt in the mouth, lubricated the throat, and gave relief after the burning.”

The cautery is best employed at a dull red or black heat, and a week should elapse between the applications, to allow the white eschar to disappear. The application should not be made at random all over the pharynx, but each spot which is thickened must be sought out and separately touched, and for this it is convenient to have cauteries of different shapes. The number of applications vary from three or four to ten or twelve.

LIGATION OF THE FEMORAL, PROFUNDA FEMORIS, COMMON FEMORAL, OBTURATOR, EXTERNAL ILIAC AND COMMON ILIAC ARTERIES, FOR POPLITEAL ANEURISM.

A man under the care of Professor Agnew (*Philadelphia Med. Times*, Nov. 10, 1877) suffered from a large aneurism in the left popliteal space. Esmarch's bandage was tried, and then digital compression was tried for twenty hours; but as the patient was intolerant of pain, the femoral artery was tied in Scarpa's space with double catgut ligature. Nine days afterwards there was hæmorrhage from the wound, and an ulceration of the artery was found in the coats of the vessel, a short distance above the ligature. The vessel was tied above and below the ulcer. On the 28th May, seven days later, there was again profuse hæmorrhage. There was ulceration in the coats of the profunda femoris, which was tied above and below the seat of hæmorrhage. The aneurism was meanwhile diminishing in size, but on June 11th a fresh bleeding took place. The vessel was ligatured above the origin of the profunda femoris, and the bleeding point was tied in the wound. The ligature separated from the common femoral on the 22nd June, and on the 23rd there was severe hæmorrhage from the wound. The external iliac was now tied with catgut ligature. On the 24th bleeding occurred from the wound in which the common femoral was tied. The flow seemed to be from the obturator. The bleeding point was secured by a ligature, with the aid of Horner's awl. On the 27th June there was bleeding from the obturator where

the ligature was last applied, but it could only be controlled by pressure. Dr. Hunter now tied the common iliac. On July 2nd there was again bleeding from the obturator, which was again secured. Next day there was hæmorrhage from the wounds in which the iliacs were tied, and the abdominal tourniquet was applied, but the patient died in the evening. At the *post mortem* examination a clot was found in the common iliac above the ligature for a distance of one inch and a quarter. About three-fourths of an inch above the ligature there was found a perforating ulcer in the wall of the vessel about three lines its diameter, from which the hæmorrhage had come. A pipe was then put in the left common iliac, and the ligature was removed from the obturator. Now upon injecting fluid it freely escaped from the obturator, showing that the left obturator artery was supplied by anastomosing branches between the right and left internal ilæc; organised clots lined the aneurism. The patient had syphilis ten years before.

SURGICAL ENGINE.

One of the newest surgical contrivances is an engine devised by Dr. James E. Garretson, and described in the *Philadelphia Medical Times* of Nov. 10, 1877. The engine is a single upright, with an arm springing from the top at right angles. It has a fly-wheel, a driving pulley, and a cord. The arm is movable in every direction, being composed of a flexible wire-shaft and a flexible sheath. The extremity of the arm is a mandrel. The capability of this arm to respond in its motions to the slightest impulse of the operator's finger, even while the instrument held by it is being whirled one or two thousand times in a minute, is little less than wonderful. Drills of various shapes, small circular saws, &c., may be fixed into the mandrel or carrier, and revolve with it. Being an invention in dentistry proper, the present common use of the machine is the preparation of the cavities of carious teeth preliminary to the process known as filling. In the treatment of osseous caries, especially about the jaws, the help afforded by it is simply invaluable. In trephining and many other surgical procedures it is said to offer the greatest advantages.

INTERNAL URETHROTOMY.

At the meeting of the British Medical Association in August last, the subject of internal urethrotomy received special attention. Mr. Teevan declared it to be the "stock operation" for stricture,

and mentioned that he had performed the operation thirty-three times without a single death. In 1,192 cases which he had collected, there were only ten deaths, or a mortality considerably less than one per cent. No other operation for stricture could be said to produce such favourable results. He recommended that a catheter should not be passed after the operation. Mr. Lund, the president of the section, also advocated the procedure, observing that a single incision was better than four or more, as only a single cicatricial splice remained. Mr. Berkeley Hill was of opinion that the operation was attended with best results in the penile portion of the urethra. It was in that locality emphatically a safe procedure. Sub-pubic strictures, on the other hand, when but moderately developed, most slowly contracted after gradual dilatation, and were generally maintained of full patency by the passage of a bougie once a month or quarter. Severe strictures in this region depended generally on a large production of fibrous and elastic tissue. Such narrowings were very slow to dilate, and in doing this great local irritation and general distress were often caused. Incision was most suitable in such cases, to be done internally when possible. The strictures that might be properly cut were the resilient or rapidly re-contracting strictures, and the narrow tortuous ones, where a large amount of fibrous tissue existed.

There was a general approval of Maisonneuve's urethrotome, or of some modification of it, but it was objected by some that the whole urethra was split by the former instrument.

CURE OF RETENTION OF URINE FROM ENLARGED PROSTATE.

Bottini (*Centralblatt für Med.*, 1877, p. 519, *Von Langenbeck's Archiv.*, and *Philadelph. Med. Times*) advocates the removal of the hypertrophied prostate by the galvano-cautery, in cases of interference with the flow of urine. Either the whole gland should be burned away, or simple division of the enlarged portion should be practised. The cautery resembles Mercier's prostatic catheter. It consists of two brass wires, fastened to a staff, and entirely insulated by a covering of ivory. Near the angle of the concave side is the cauterising apparatus, a V-shaped piece of platinum two and a half centimetres (one inch) long, of which one limb is connected with the anterior wire of the instrument, the other with the posterior. As soon as the loop of the instrument can be moved about in the bladder, which should be partly full, it is to be brought against the hypertrophied part by a movement through an arc of

180°, and thus, surrounded as if by a hook, the structure can be destroyed with the utmost precision without (as *post mortem* examination has shown) disturbing the neighbouring parts in the least. The thermo-galvanic incisor is like a lithotripter, of which the small blade is formed of a platinum knife. This is connected by a bit of copper to the staff, and glides in the glass groove of the female blade. The point of the instrument must be applied with its concavity pressing against the lobe to be divided, so that the latter is enclosed as in a book. Neither the cauterisation nor the cutting causes much pain. The bladder is usually emptied shortly after the operation, though strangury is ordinarily experienced. No bad effects are observed—even vesical catarrh has never been noted. The urine, however, is frequently slightly bloody for a short time. Cauterisation is usually advisable in partial and not prominent enlargements both of the supra-collicular portion and the lobes of the prostate. Division is to be recommended in general and uniform enlargement of the gland, and also in very prominent intumescences. Contra-indications are—inactivity of the detrusor, abnormal condition of the urine, coincident renal disease.

ABSORPTION OF THERAPEUTICAL SUBSTANCES BY THE VAGINA.

DR. E. W. HAMBERGER has lately published a number of experiments with the view of determining the absorbing power of the vaginal mucous membrane. A tampon impregnated with a solution of the medicinal substance was introduced into the vagina by means of a Fergusson's speculum, and retained for twenty-four hours by a dry tampon to prevent the backward flow to the vulva. The urine passed from time to time was examined for the drug thus administered. Iodide of potassium was found two hours after the introduction of the tampon, and traces of it remained twenty-four hours after removal. Ferrocyanide of potassium and salicylic acid appeared three hours after they were thus given, and bromide of potassium took the same time to appear. Hamberger believes that the administration of drugs by the vagina can be employed in all cases of obstruction of the normal ways, and in gynæcological practice he considers it will be specially useful.—*La Province Médicale*.

S. W.

PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

FORTIETH ANNUAL SESSION.

Saturday, 17th November, 1877.

MR. DARBY, President, in the Chair.

President's Address.

GENTLEMEN—This evening brings the thirty-ninth year of this Society and my year of office to a close. Before I vacate the chair, to which you have done me the honour to elect me, I must ask you to accept my best thanks, and to allow me to say that when I bear in mind the fact that I am the only physician residing and practising out of Dublin who has been so highly honoured, the compliment is, in my estimation, materially enhanced.

During the past year we have held eight reunions. At the first meeting Dr. Atthill delivered an interesting and eloquent address, and I must now endeavour, though with halting steps, to follow in his wake. At the subsequent meetings Dr. Atthill brought forward, besides his important report of the Rotunda Hospital, many cases of great interest—amongst others a case of *post partum* hæmorrhage, which led to our having the advantage of hearing, from Dr. Robert M'Donnell himself, a description of the instrument invented by him for the transfusion of blood, and of the manner in which he applied it when performing that operation.

We had also—

Dr. H. Kennedy's paper on "Croup."

Dr. Boyd's paper on "Syphilitic and other Tumours affecting the Genital Organs."

Dr. Wilson's (U. S.) paper on "Strapping the Breast."

Dr. Purefoy's "Case of Extra-uterine Fætation."

Dr. Denham's "Recent Specimen of Uterine Polypus."

Dr. Finucane's "Cases of Erysipelatous Metritis."

Dr. Doyle's paper on "Stimulants in Certain Cases of Uterine Disease."

Dr. Kinkad's communication on "Diphtheria."

Dr. Kidd's and Dr. More Madden's "Cases of Vesical Calculus in the Female."

Dr. Macan's "Case of Amputation of the Uterus."

Dr. Churchill's paper on "The Use of Scissors in the Removal of Uterine Polypi."

Dr. Russell's "Case of Puerperal Convulsions."

Dr. J. A. Byrne's "Case of Uterine Fibroid and of Intestinal Constriction."

These papers, brought forward at our meetings during the past session, afford abundant evidence of the interest taken in our proceedings, and of the vigour with which the Society accomplishes the objects for which it was founded.

Dr. Finucane's paper gave rise to a discussion into which the term "blood-poisoning" was introduced, and I find, on looking to the report on that discussion, published in the April number of the *Dublin Journal of Medical Science*, page 378, that some remarks made by me, from this chair, have not been accurately reported.

On that and on other occasions I have declared my inability to understand the doctrine of Blood-poisoning, or Septicæmia—terms "familiar in our mouths as household words," implying, as it appears to me, that the blood takes up a poison, putrid or specific as the case may be, both by direct inoculation and by some other means, which it carries and distributes through the system. This is a doctrine I cannot understand; and, as these terms are frequently applied to puerperal disease, I feel that in addressing some remarks to you upon this very important and difficult subject—which, indeed, may be called the subject of the day—I am addressing myself to an appropriate tribunal, fully competent to deal with the subject, and one which will, I am sure, deal with my deficiencies in a friendly spirit.

I shall first proceed to the consideration of disease produced by inoculation, requiring inflammation for its development, and then refer to essential disease in the development of which inflammation plays no necessary part—in conclusion stating the doctrine which I myself hold upon the subject.

I. For an example of the first of these we may take the case of a dissecting wound where the matter inoculated is putrid, and the inflammation which follows is diffusive, irregular, and undefined. Other examples may be seen in the inflammation resulting from the specific virus of syphilis and of vaccinia which is tolerably regular in its course, and definite.

I would ask, do these cases exhibit any sensible evidence, throughout their whole course, of the blood being putrid?

Though not exactly coming under this head, I may here adduce the case of gangrene where the affected part ferments, evolves gas, and becomes sensibly putrid, while still retained, not only in immediate contact with, but actually continuous with the surrounding living tissues. Even here the blood in circulation is not sensibly putrid. On the contrary the gangrenous part is sloughed away and has evidently no real connexion with the living tissues though in contact with them; were it otherwise, and if the putrid matter were spread through the blood, it would reach the heart and kill at once. In fact, the gangrenous part ceases to be a living portion of the body.

II. Of the second class of diseases to which I would refer—namely, those essential diseases, in the development of which inflammation plays no necessary part, I take for examples fever, cholera, and purpura. In these the blood may be considered as especially implicated, and yet, in the course of my observation of them, I have not been able to discover fermentation or putrefaction in the blood analogous to the fermentation and putrefaction which we observe in dead organic fluids.

And I would here remark, in reference to deductions drawn from experiments upon these dead fluids, that they cannot be fairly extended to anything taking place in the blood, as it is in the living body, the fact being that there is no true analogy between the cases, as the experiments are conducted under totally different conditions. Even where we follow the diseases I have referred to under this head to the dissecting room, the blood presents no sensible difference from that which it does in death from other diseases, whereas if those processes were going on they must become sensible.

In proof of this I would call your particular attention to that awful disease, named by Stokes "*Acute Purpuric Fever*," and aptly compared by Dr. Lyons to the "*Black Death*" of the middle ages. In this disease the truth that "*a little leaven leaveneth the whole lump*," is painfully exhibited.

According to my observation of it, putrefaction of all the tissues sets in with such appalling rapidity that it may be said that the person attacked by it has scarce time to die before his whole body has become sensibly putrid. Yet I believe that, had we the means of ascertaining the fact, the blood would be found to be the last thing to putrefy.

I have adduced this case, in contrast to the illustration from gangrene, in further proof that, where putrefaction actually does take place in the living body, it kills at once.

It is not my intention to discuss the theory of spores in the air, or of bacteria in the blood, but it is right I should state that, in all the cases of disease mentioned in this paper, the patients were exposed to the

atmosphere in different places, and one case of acute purpuric fever occurred in a well-ventilated hospital ward where nine other patients were at the same time suffering from common continued fever.

III. I come now to state, in conclusion, the doctrine that I hold myself upon the subject.

Let us take the case of a man who drops dead as he is walking in the street. What do we find here? In a single minute the following, amongst other remarkable changes, have taken place:—In the first place, just as we can, by cleanliness, check putridity in foul discharges from wounds, so we can produce or retard fermentation and putrefaction in the dead body, which I hold to be impossible in the living man. In the second place, we cannot produce inflammation or fever in the dead body, while both might have been induced in the living man.

What, I ask, produces these differences? Is it not evident that if the causes be merely physico-chemical, the same changes ought to be possible of production both in the living and in the dead body. Whereas if we assume that the living body is under the influence of a higher force, by whatever name it may be called, the cessation of that force in death sufficiently explains the phenomena that follow.

In accordance with this, instead of attributing the power of producing disease to the blood receiving poison, and carrying and distributing it through the body by a sort of hydraulic action, I attribute it (the communication of disease) to the affection of the whole vital susceptibility. When, for example, I vaccinate a child, the effect is produced not by the blood distributing the vaccine poison mechanically through the system, but by the combined and associated action of every sentient atom of the part, solid and fluid, recognising and receiving the impression from without and conveying that impression to the whole system, by which is conferred immunity from the infection afterwards.

Similarly when a man takes a fever, whether through the skin or through the mucous membrane, he has not to wait for fever until the blood has carried it to each part of his frame, but the whole man by a simultaneous shock has received it through the affection of vital susceptibility.

The conclusion at which I arrive is that the terms blood-poisoning and septicæmia imply that which is not the fact, and lead to erroneous practice on the false supposition that there is a condition of putridity in blood in circulation. Finally, I would maintain that where disease or injury is inflicted on the body, its organic life is assailed, its vital susceptibility recognises, resents, and resists the assailant; and, gentlemen, it is the mission of the physician and of the surgeon to come to the rescue and fight on the side of assailed vitality.

Saturday, 8th December, 1877.

On the Treatment of Post-partum Hæmorrhage by the Injection of Hot Water into the Uterus. By LOMBE ATTHILL, M.D.; Master of the Rotunda Hospital, Dublin.

Post partum hæmorrhage is of such frequent occurrence, and so often assumes an alarming character, that any method of checking it which combines efficiency with ease of application and safety to the patient is certain to be hailed with satisfaction by practitioners. Without doubt the most efficient means at our command for the arrest of flooding after labour is the injection of a styptic, such as the solution of the perchloride of iron, into the uterus. This is a procedure which, after repeated trials, I have no hesitation in recommending, and I shall continue to have recourse to it in suitable cases. Apart, however, from the alleged danger of injecting a powerful styptic into the uterus—a danger which, though well-nigh groundless, suffices to deter many from having recourse to it—there is this objection to the practice, that the perchloride may not always be at hand when the emergency arises, and that valuable time may be lost ere it can be obtained.

The introduction of the hand into the uterus—in some cases an efficient method of checking *post partum* hæmorrhage—is certainly not free from danger, and is moreover by no means reliable in its results. While the routine treatment by cold, whether applied to the surface or injected into the uterus, requires for its success that the patient be possessed of sufficient vital energy to ensure reaction. In other words, the application of cold in *post partum* hæmorrhage is a most efficient remedy in cases where a sudden loss of blood occurs in an otherwise healthy woman, who has not been exhausted by an unduly prolonged labour; but is altogether unreliable, and in many cases positively injurious, where the patient has been debilitated by previous disease, worn out by long protracted suffering, or exhausted by frequent, though it may be small, losses of blood.

As far as my personal experience goes, those apparently alarming losses of blood which sometimes occur immediately after the birth of the child, or expulsion of the placenta, are not likely to terminate fatally; they can in general be at once arrested by steady pressure over the fundus of the uterus, and by the use of cold, but the hæmorrhage to be dreaded is that in which the blood trickles away in a little never-ceasing stream, the uterus relaxing and contracting alternately. This form of hæmorrhage, of which I have seen several fatal cases, is most liable to occur in debilitated women, and, in such cases, cold is in general absolutely useless—nay, more, often injurious. I shall never forget a case to which I was called several years ago. The patient had been confined prematurely some hours before; subsequently, hæmorrhage set in, not profusely, but it could not be checked by “the usual means,” and her

attendant becoming alarmed, I was sent for. This patient had been for a long time assiduously treated by the application of cold. Napkins wrung out of cold water had been continuously applied to the vulva, and to the abdomen, cold water injected, &c., but all in vain, a little stream of blood never for a moment ceased to trickle from the vagina, and yet the uterus was firm. When I saw the patient, the whole surface of the body was cold, and the pulse could hardly be felt. My first step was to remove the wet sheet and dripping napkins, and to apply warm jars to the extremities, and I had the satisfaction of speedily seeing the hæmorrhage cease, without any further treatment than friction to the fundus of the uterus. From this case I learned that the prolonged use of cold in cases of *post partum* hæmorrhage may be absolutely injurious, and my practice has been to have recourse to other means if, after a fair trial, cold produced no effect, and not to defer doing so till the vital powers were too depressed.

Another practice I have had recourse to with good results, in certain forms of uterine hæmorrhage, not connected with pregnancy, has been the use of Chapman's spinal hot water bags; and, reasoning from the benefit derived from them, in these cases, I thought of trying them in *post partum* hæmorrhage, but from one cause or another I never carried out my intention. My views being thus somewhat unsettled as to the possible value of heat in *post partum* hæmorrhage, I hailed with satisfaction the suggestion to inject hot water into the uterus in such cases, and decided to put its value to the test of personal experience.

Having decided to try the method, the opportunity was not long wanting. On the morning of the 20th November, a woman aged thirty-three was delivered in the Rotunda Hospital of her fourteenth child. Labour had been easy and natural, and pressure, as is the usual practice in this hospital, was maintained over the fundus till the placenta was expelled, which occurred in about fifteen minutes; profuse hæmorrhage set in immediately after, and Dr. Smyly, Assistant-Physician to the Hospital, was sent for; he applied pressure to the fundus, cold to the vulva, and injected cold water into the uterus with good effect, but the patient becoming alarmingly weak, he sent for me.

On my arrival in the ward, she was almost pulseless, the face pale, and the surface of the body cold. There was little hæmorrhage going on, but the uterus relaxed in spite of pressure with the hand on the fundus, and a little stream of blood continuously trickled from the vulva. She was in a state of great danger, and in a condition which would have warranted the use of the perchloride of iron, but instead of having recourse to it, I resolved to inject hot water; this was procured in a moment, and passing the tube of the syringe right up to the fundus of the uterus, I injected water freely at the temperature of 110°, keeping my hand at the same time over the fundus. I was pleased to find that

the uterus contracted firmly under it, exactly as it would close had I employed the perchloride of iron. In a very short space of time, probably before I had injected more than a pint of the hot water, the fluid ran nearly clean from the vagina, the pulse improved markedly, and I ceased to inject any more. After a short time the binder was applied; no further bleeding occurred, and the patient made a rapid and good recovery.

On the 30th November another opportunity offered. A young woman, rather pallid and delicate looking, was delivered, at 8 45 a.m., of her third child. No hæmorrhage occurred at the time, and she appeared to be all right; but, at 10 30 o'clock she complained of feeling weak, and was found to be losing blood. Dr. Smyly saw her promptly, and, on removing the binder, found the uterus to be relaxed and flabby, and to reach to above the umbilicus. On using pressure an enormous quantity of clots were expelled, and blood flowed freely from the vagina. Cold was applied, and the uterus contracted firmly. I now saw her; she was very pale, and the pulse could hardly be felt; the uterus was firmly contracted, but remained very large. As the hæmorrhage seemed to be checked, I did not at first think that more need be done, but while my hand was still on the fundus I perceived it to relax, and blood flowed again. I therefore decided on injecting hot water before matters became worse, for it was evident that she could bear very little further loss.

On introducing my fingers up to the os, with the view of guiding the tube of the syringe into it, I ascertained that the great size of the uterus was partly due to the presence of a fibrous tumour in its anterior wall. This fact did not deter me from proceeding with the injection, but rather decided me to do so. As in the previous case, the water no sooner reached the fundus than the uterus contracted firmly, and the oozing of blood ceased; but the most remarkable feature was the immediate effect on the pulse—it at once improved markedly, becoming fuller and stronger. The uterus did not relax again, and no further loss occurred. This patient recovered strength very slowly, but was able to get up in a week. On questioning her subsequently as to her feelings at the time, she stated that at the moment of the injection she experienced the greatest comfort, and obtained immediate relief from intense pain from which she had up to that moment suffered.

I am unable to say to whom we are indebted for the introduction of the use of hot water injections for the control of uterine hæmorrhage. I was induced to try it in consequence of a letter written by Dr. Whitwell, of San Francisco, to Dr. Foley, of Boston, who is at present studying in this hospital. Dr. Whitwell's statement is to the following effect:—When house surgeon at the New York State Women's Hospital, he saw the uterus contract firmly and instantly upon being washed out with hot water after an operation by Dr. Marion Sims, upon a sarcomatous growth of the fundus uteri. This was in 1874. The result led him to try the

same treatment in *post partum* hæmorrhage, where he met with perfect success. He afterwards succeeded in having the treatment tried in the Lying-in Hospital at Prague, where he studied for some time. At first he was ridiculed, but as the method was successful in every respect it was adopted as a regular routine treatment. The water should be at a temperature of 110° . I had previously seen the treatment alluded to in some of the periodicals, but this letter decided me to test it personally.

The first notice which I have been able to trace relative to the use of hot water injections in *post partum* hæmorrhage, occurs in the *American Journal of Obstetrics* for April, 1876, in which, in an abstract of a paper by Dr. Carl Von Rokitsansky, jun., "On the Treatment Employed in Vienna for Uterine Hæmorrhage," the following brief passage occurs:—"Dr. Windelband has recently recommended injections of hot water in menorrhagia and *post partum* hæmorrhage," and nowhere else have I been able to meet with more explicit directions to the subject.

My experience is as yet but scanty, still I have twice used hot water injections in cases of sufficient gravity to call for the most prompt and active treatment, and with most satisfactory results. Both were examples of that class of *post partum* hæmorrhage which are liable to end fatally, not indeed immediately, but in the course of a little time, if the thin stream of blood be not stayed. Since I became Master of the Rotunda Hospital two such cases ended fatally.

The cases I now record undoubtedly establish this much—that the injection of hot water powerfully stimulates the uterus to contract, and thus rapidly checks the hæmorrhage; but that it does more is, I think, as clearly established; it evidently acts as a general stimulant. The effect on the pulse was most marked, indeed the pulse was effected more rapidly than by the hypodermic injection of ether, and it did not flag again. The faces of the patients, too, lost the deadly hue they previously had worn—and last, not least, they expressed themselves as having experienced the greatest relief, and obtained great comfort.

I anticipate very good results from the introduction of this simple treatment into obstetric practice. There are many men who fear to use the perchloride of iron as an intra-uterine application, but who, when prejudice has been overcome, will not shrink from the use of hot water; and, I believe, not a few lives may thus be saved. To recapitulate, in hot water we have at once a safe and efficient remedy, one comforting and agreeable to the patient, and an agent which is always at hand. Whether it is as reliable as the perchloride of iron remains yet to be proved.

It should be remembered that the advantage to be derived from the intra-uterine injection of hot water is not confined to cases of *post partum* hæmorrhage. It was first used to check hæmorrhage occurring in cases of chronic disease of the uterus, and after operations. Its range of

usefulness is therefore great. At the present time I am engaged in observing its action in chronic cases, and hope, in time, to be able to bring forward the results.

In conclusion, I would urge practitioners to try for themselves the use of hot water injections. I can assure them that they need not fear bad results; nor, though 110° is the temperature named, is accuracy in this respect needed. Water in which the hand can be kept without discomfort may, with safety, be employed; but, it must be remembered, that if the temperature be allowed to fall much under 110° , disappointment will certainly follow; equally will the injection be well-nigh useless if the tube of the syringe be not passed right up to the fundus of the uterus, or at least fairly to within its cavity.

TRACHEOTOMY IN TUBERCULOSIS OF THE LARYNX.

WE are accustomed to consider tracheotomy in laryngeal tuberculosis simply as a possible means for prolonging life for a short period. Dr. Seckowski, however, is of a different opinion. He has operated twice for tuberculosis of the larynx, and one of the cases on whom the operation was performed seven years ago is still alive, whilst the other lived for three years. *Post mortem* examination of the latter showed well-advanced phthisis. The one still living was attacked with severe dyspnoea immediately after her return from a long journey. The writer resorted immediately to tracheotomy, when, after the introduction of the cannula, his patient fell into a natural sleep which continued for forty-eight hours. Under general treatment her strength was regained and her cough left her, but she continued to wear the cannula most of the time for two years—that is, until laryngoscopic examination showed that the former morbid condition had left behind only a thickening of the vocal cords. Two years later there was still marked dulness to percussion over the apex of the right lung. Since that time she has never been examined, but the writer often sees her in an apparently well-nourished condition. He expresses the opinion that the opening in the trachea was not only of temporary benefit, but that it prevented the extension of tuberculosis. He considers it necessary that the opening of the glottis should be sufficiently large to allow the easy expectoration of purulent secretion from the lungs, as well as the entrance of plenty of air. He therefore believes tracheotomy to be indicated in all contractions of the larynx, particularly in tubercular patients, for it saves the larynx as well as the lungs. It would not seem to be indicated in those cases in which the lungs are more affected than is the larynx.—*The Clinic*, September 22, 1877, and *Phil. Med. Times*.

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

SESSION 1877-78.

President—G. F. WALES, M.D.; F.R.C.S., Ed.

Hon. Secretary—WILLIAM WHITLA, M.D.

Wednesday, the 14th day of November, 1877.

The President in the Chair.

THE PRESIDENT read his opening Address as follows:—

GENTLEMEN—In entering on the presidential duties of the session, let me thank you for the honour you have conferred on me—an honour which was unlooked-for, and which I value the more because of its coming from you so unanimously. It is gratifying to a man after a life's labour to find that he is considered worthy of such a spontaneous tribute of respect, for next in satisfaction to that afforded by the *mens conscia recti* is that resulting from the approbation of one's professional brethren. I wish I could feel that I really merited such favourable consideration. I can but strive to prove in some degree worthy of your kindness.

I have to acknowledge gratefully the many promises of co-operation in the work of the Society from members generally—from some of my respected predecessors in office, and from my seniors in the profession. With such support, and your indulgence for my shortcomings, I hope, aided more immediately by the staff of the society and its able and indefatigable Secretary, to see its usefulness preserved, and, if possible, extended in the coming session.

It is now more than a quarter of a century since I entered on the duties of my profession and joined the old Medical Society of Belfast. About this time men began to shake themselves out of the old grooves of thought. Physiological, pathological, and chemical research took on more activity, a spirit of inquiry and criticism spread abroad, leading not only to a disposition to break new ground, but also to test the foundations of received opinions and established practices. Amongst the foremost in this town to catch the spirit of the time was the late Dr. Malcolm. He founded the Pathological Society here, not to supplant but to supplement the Medical. I had the honour of acting with him as joint-secretary during the first year of its existence. It turned out remarkably successful. Its meetings

were held weekly, and were well attended by members of the profession, old and young, from town and country. Queries were regularly propounded for discussion, and formed a prominent feature in the working of the Society. These always excited interest, the members generally taking part in the discussions, which were published. No man ever did more for the profession in Belfast while he lived than did Dr. Malcolm. Like a little leaven he leavened the whole mass with the spirit which animated himself, but he died too soon, and left a void in our ranks which was long felt. After his death the Pathological Society maintained its usefulness for a time, but gradually it waned, and finally merged with the Old Medical into the "Ulster Medical Society." We thus combine the two, and considering that much of the old matured element is still with us (a consideration which inspires me, in passing, to hope that former presidents will occasionally manifest by their presence and countenance the interest they once felt and promised in our welfare), and considering the superior character of the new element, it will not reflect creditably on either if we have a sluggish and unprofitable session.

In closing this special reference to our Society, it is with unfeigned sorrow I notice, at this our opening meeting, the absence of a once familiar pleasant face. I refer to the late Dr. John M'Crea. We will miss him in the work of the Society, in which his ability, attainments, and cultivated experience made him so valuable; and we will miss him otherwise, for the high tone of honour and genial fellowship which characterised him and made him a favourite.

The last quarter of a century has been a period of transition and progress. No similar period in our history has been so eventful, so revolutionary, so humbling to assumption of power, and yet so satisfactory in point of work done. At the commencement of the time referred to we still pursued the vigorous methods of combating disease which belonged to the previous generation, chief of which was blood-letting. In my early years few days passed without my using the lancet, and yet it is now several years since I performed venesection. At first sight it will appear either that we were wrong then or that we are wrong now. I believe the lancet was too much used before, and is unjustifiably neglected now in deference to modern views. I can well recollect the great and permanent relief so frequently given by the lancet in acute serous inflammations, pneumonia, and congestive head affections; and yet it is so forgotten now that I venture to say a great many of our younger brethren have neither bled nor seen bleeding throughout their whole career. Some explanation of the difference in practice at the two periods is afforded by the recollection that formerly it was quite the habit to be bled annually. However such an injurious practice came about I do not know, but it begot a plethoric condition which maintained the demand for periodic depletion, owing to the relief that always followed. About

the time that bleeding began to be called in question, or rather a little prior to it, very active medication was as much the rule as bleeding, so that patients, I may safely say, were seldom lost by inaction; but with the growth of homœopathy there came a change in our views. Notwithstanding all the irrational assumptions of that system, it secured many adherents amongst the better classes. It fascinated them by the simplicity of its law, by the possibility of their grasping the symptoms, and selecting easily the assumed remedy by a reference to its alleged effects. It satisfied thus that craving for amateur doctoring which so universally prevails. Then, again, the sugar globules and fanciful dilutions were not unpleasant to taste like our medicines. They did not sicken, and whatever other effect they had or had *not* was supplied by the imagination. But under homœopathy and hydropathy, another rival system of therapeutics, it was found that patients did very well. Medical men were not slow to discover the truth. They perceived that nature could be safely trusted to manage a good deal of her own business, and that she did manage it in every case treated homœopathically, except so far as the admirable dietetics of that system, and the mental impression contributed to the result.

The lesson had its influence. It was found that disease could be dealt with without such active drugging as had been in vogue, and faith and empiricism gave way largely to scepticism and rationalism. The reaction from the excessive use of drugs, like bleeding, led, in the tendency to extremes, to much unjustifiable neglect of therapeutics.

It is right to be as rational as we possibly can in the treatment of disease, but the practice of medicine must always remain largely empirical, and we must not undervalue it on that account. The experience of our fathers should receive our most respectful consideration. If they had not the same light to guide them that we have now, it did not hinder them from doing good work. They certainly were great in powers of observation. They have portrayed disease for us, in nearly all its phases, with singular truth and fulness, and they have given us the great bulk of the medicines we possess, with an accurate description of their effects. That was their work. Ours is to develop the causation and pathology of the one, so as rationally to utilise the other. In other words, by an improved knowledge of the causes and conditions of disease, to select remedies that will apply to them in preventing or mitigating their operation rather than such as may only influence the symptoms and signs. The more we can succeed in doing this, the more scientific and rational will be our practice. That we are gaining sure footholds in the path of knowledge, and making rapid advances cannot be doubted. True these advances are sometimes made at the sacrifice of much that was considered well established; for example, it is disconcerting to be told that the idea of a limited brain area for each special sense is no longer tenable; that if any such

localisation exist, it exists in cells diffused pretty generally through a large extent of brain substance. We can no longer say in a case of aphasia, anæsthesia, or motor paralysis, that the lesion is absolutely so and so, for we know that, apart from any lesion, mere irritation, remote as well as direct, will induce any or all of these; and, further, we know that such conditions may be maintained indefinitely by some inhibitory influence, often slight, the removal of which will again bring into play the functions of the uninjured generating and conducting nervous structures. In point of practical value I believe that no investigations of late excel those made by Professor Lister. It is a great step in knowledge to know that animal fluids and secretions have not any inherent tendency to putrefy—that they will remain in contact with pure air—*i.e.*, air freed from living germs, without change; that such change when it takes place is a true fermentative process—*i.e.*, a change due to the growth and development of living germs. I think the Professor has absolutely demonstrated these points by his recent experiments, and that those who have asserted that the ferment (so called) may be a chemical and not a vital agent, have failed in their proofs. Great as have been the advantages resulting to surgery from the application of this knowledge, I believe that greater will be realised in medicine when the various morbid blood conditions come to be better investigated.

Having lightly touched on questions peculiarly our own, there remains one which is of general importance and interest, and which, I think, from the increasing magnitude of the evil, we, as guardians of the public health, have not only a right to speak out on, but I think we are morally bound to do so. I allude to the growing evil of intemperance. I cannot conceive any subject more important or more worthy of our earnest consideration. I am sure I am only speaking your experience when I say from my own that amongst men, and not inconsiderably amongst women of all classes, the use of strong drink as a beverage has produced much of the disease we have had to treat, not to speak of greater evils even than disease. Speaking for myself, I would say that alcohol in any form is unnecessary in health, and injurious, unless taken in great moderation; that when so taken it should only be in connexion with food, and not before dinner; that those who have not been accustomed to it should not lightly begin it; that youth should take none. It is difficult to see how young persons can be preserved as long as stimulants are habitually used at table. I was much impressed while in the United States recently by observing the general absence of liquor on the tables in the dining halls of the large hotels. Persons who wished for drink had to go to the bar for it. This was a great protection to youth and weak persons of both sexes, and might be imitated with advantage amongst ourselves. Beyond the class likely to be in any degree influenced by suggestions from us, there are the great classes which will never move

to save themselves. They resort to public-houses, not to refresh, but to get wholly or partially drunk. If it is wrong for the respectable man to drink before dinner, why should the poor wretches who have lost self-control in the matter of drink be allowed to drug themselves to ruin and death, either for the profit of the State or individuals? Things are not right. Let the clergy speak out boldly, and tell their people of the moral degeneracy that drink is producing. Let our legislators and magistrates endeavour, not less to diminish the facilities which they have created and sanctioned, than to punish those who, tempted by such facilities, become victims. Let philanthropists generally who desire to accomplish the greatest good for their fellows and their country, try to save the rising generation from intemperance, protect the weak, and render the irreclaimably inebriate as harmless as possible. Let us aid the philanthropists and others, by our influence and advice, and by our declaring what we know of the effect of alcohol as a beverage. With its use as a medicine in our hands the public have nothing to do, and would wisely take nothing to do. Our experience will guide us better than theorising and assertion. The whole question is on the paper for discussion. One of greater gravity and importance there cannot well be, and I commend it, gentlemen, to your earnest consideration, with the view to some practical issue in the public interest.

Discussion on Alcohol.

DR. JOHN MOORE then opened a discussion on "Alcohol" as follows:—Is the moderate use of alcohol beneficial or injurious to the healthy human system? How much hangs on a true answer to that question! That the immoderate use of alcohol is injurious there can be but one opinion; but that men believe in the beneficial effects of the moderate use of alcohol is testified by how much they pay for it—millions of money, oceans of tears, thousands of desolated homesteads, and countless broken hearts are freely flung into its treasury, as the price paid for the beneficial effects supposed to arise from its use when taken in moderation.

I may state at the outset of my remarks that I am not competent to enter upon a discussion of those experiments which have been carried out by practical physiologists and expert chemists on the action of alcohol on the elementary tissues of animal bodies. I may accept the teaching on these points of those fitted to deal with these subjects, and confine myself to the empirical experience gathered over now many years.

The subject presents almost an insurmountable difficulty at the very outset, as we have no "standard of health" to which we can refer—the different amounts of energy or vitality is so varyingly bestowed. One can walk a thousand quarter miles in as many consecutive periods of ten minutes each, and be vigorous at the end, while another would break down under the tenth of the task. On some the climatic state has a

powerful effect—bright sunshine, clear frost, brisk breezy days producing a glow and imparting energy, buoyance, and cheerfulness, whereas dark, gloomy weather sheds the same sombre hue over the spirits, and depresses vitality to the lowest point of health.

The construction of the nervous organism in some is formed on such a high key that every thrill that sweeps across it causes it to vibrate with hyperæsthetic sensations; these know how

Dearly-bought the sudden treasure
Finer feelings can bestow,
Chords that vibrate sweetest pleasure
Thrill the deepest notes of woe.

These are the individuals more susceptible of the influences of alcohol, and more in danger of being drawn into its snares. Others, again, are so anæsthetic that what to the former class would be torture they bear with calm placidity; in these the resisting power is great.

The “moderate” use of alcohol presents the same difficulty; it is an elastic measure, not a fixed standard. Where does it begin, and where does it end? Moderation in alcoholic stimulants to one individual is downright drunkenness to another. I know some on whom a single glass of sherry has such an effect as to be quite apparent, and I know others who could carry a bottle, and yet manifest no symptoms of being overburthened. On endeavouring to analyse one’s own feelings and sensations on partaking, when the appetite has been sharpened by outdoor exercise, of a tender, well-cooked beefsteak (accompanied with its usual accompaniments, by mealy potatoes with good bread)—these, aided with a bottle of Bass or Allsop’s, produce a pleasure in the participation, a feeling of satisfaction afterwards, and an amount of invigoration following as to make one hardly care to call his king a cousin. The intoxicating effects of the tumblerful of beer is quite perceptible, and lasts for a few minutes; it can be completely dissipated by a mental effort, and can be reinduced by yielding to a relaxation, inducing a dreamy, pleasant mood. That is very evanescent. On substituting a small quantity of claret for the beer, the most careful intra-inspection cannot detect a trace of the psychological action of its alcohol. I know others on whom a single glass of beer has the same effect, or still greater, paralysing not only the vaso-motor nerves, as seen in the flushed face, but also the voluntary nerves, in loss of co-ordinating power over the movements of the tongue, and their speech betrayeth them. Where can be drawn here the line of demarcation? Does the glass of beer cross the limit of moderation, and the glass of claret fall short of it? Again, in order to arrive at an accurate estimate of the injurious or beneficial effects of the moderate use of alcohol, it is necessary to take into account the resisting power of the human system to noxious influence—the amount of toleration of which it is susceptible—the wide range of temperature from the Arctic regions

to the Tropics which it can bear with impunity; the miasmatic exhalation, so deadly under certain circumstances, yet when an individual becomes acclimatised, he passes through these as if he possessed a charmed life. The dissection wound that might prove fatal to a young student from blood-poisoning proves harmless to one who has become habituated to the atmosphere of the dissecting-room. The opium-eater and the laudanum-drinker swallow doses that would be lethal to others; and some of us remember the deadly sickness that crept over us as we were first trying the effects of that wonderful Virginian weed which afterwards came to exert such a soothing influence, when in the silence of twilight's contemplative hour we have mused in a sorrowful mood. A solution of the problem we are seeking is for this threefold reason rendered impossible—there is no standard of health, there is no measure of moderation, and there is no possibility of correctly estimating the resisting power of the human system to noxious drugs. It is thus placed outside the region of demonstration, and all that we can hope to do is to oscillate and circulate as near the centre of truth on this point without the knowledge of when we have arrived exactly at it.

On the 3rd of July last Dr. Thomas Lauder Brunton, Lecturer on *Materia Medica* in St. Bartholomew's Hospital, gave evidence before the Committee of the House of Lords, appointed to inquire into the subject of intemperance. In reply to query 9,249, Dr. Brunton stated that alcohol in small doses, when swallowed, somewhat increases the secretion of the stomach, and thereby aids digestion; it is then absorbed; it increases the heart's action, dilates the vessels, determines the blood to the surface, and gives a feeling of pleasant warmth and comfort.

In answer to 9,529, he says:—If a man eats well and sleeps well he does not require alcohol, he is better without it.

9,527. If a man is perfectly healthy he does not require spirits or wine; but if he is weak they are useful. If a man is working hard all day his stomach is exhausted like the rest of his body, and incapable of digestion; whereas if he takes a glass of wine first it begins to secrete the gastric juice, and his food is digested comfortably, otherwise it lies undigested.

9,356. Do you consider it equally necessary in case of great mental exertion? Yes. 9,357. I think even more so!! 9,365. Spirits taken during exposure to cold are bad, but after the exposure is over they are beneficial.

Dr. John Burdon Sanderson:—9,383. It was generally entertained some years ago that alcohol was not oxidised in the system. This has been completely refuted by very careful experiments.

9,384. An ordinary man may take as much as two ounces in twenty-four hours of alcohol, and the whole of it will be oxidised.

9,385. It is perfectly clear that alcohol does not go to build up any

tissue in the body; on the other hand, it is equally clear that it is used for the purpose of respiration. As long then as alcohol is taken in such quantities that it can be completely burnt, then it is beneficial; but as soon as it begins to accumulate it becomes otherwise than beneficial.

9,386. Alcohol cannot, like fat, be stored up in the system, but the question remains open whether it may not be converted into other kinds of force.

9,398. My belief is that upon the whole the human race would be situated as favourably if the use of alcohol did not exist. I think so for two reasons. In the first place, because the evils certainly preponderate over the benefits; and the other consideration is simply that all the benefits are dispensable benefits.

9,406. Treating alcohol as a kind of food, there is no other food which deteriorates the organs, almost of necessity, in its use; but perhaps you do not admit that alcohol does so? I quite admit that.

9,435. You would say that the amount of pleasure which is given and the amount of medical aid which is derived from alcohol are overbalanced by the disadvantages? I do not desire to be understood as saying that it is desirable that the human race should do without alcohol even in health, for it is clearly beneficial. There are two actions of alcohol on the health—that feeling of glow and increased comfort which it produces, which is a most desirable thing; and secondly, its exhilarating effect—its direct effect upon the nervous system.

On the 13th July last Sir William Gull gave the following evidence:—

9,993. I think there are conditions of the system under fatigue and exhaustion where alcohol might be useful, but I very much doubt whether there are not some sorts of food which might very well take its place.

9,996. Take the case of a man of twenty-five, sound in wind, and limb, and health, would you advise him to give up alcohol, if he were to ask you the question? I think I should consider his calling. If he were a carter, or a man occupied out of doors much, I am not sure that I would not advise him to take some beer, but I am not very positive about it.

10,005. Many people believe that intellectual work cannot be half so well done without wine or alcohol? There I join issue at once.

10,007. Would you hold the very opposite? I should hold the opposite.

10,008. Would you say that a moderately temperate person might be benefited by a slight use of wine or alcohol? I should hold the opposite as regards the intellect.

10,009. The constant use of alcohol, even in a moderate measure, may injure the nervous tissues? Yes, certainly.

10,004. I think that taking it as a whole there is a good deal of injury

done to the health by the habitual use of wine and alcohol, even in so-called moderate quantities.

10,032. I think I have stated that alcohol in its various forms has uses, but I think those uses are very much limited by the age and health of the consumer. I think for the most part good food will supply all the wants up to the middle period of life; in old age and disease you may want some artificial stimulus. I would also say I do not know how alcohol does act upon the body altogether, but in disease it is very much as a sedative.

I have thus endeavoured to give what information the greatest lights, the most brilliant luminaries of British medical science, were able to lay before the highest court of inquiry in this kingdom, and I cannot say they have added very much to our knowledge—indeed their very greatness seems as if it would lead to bewilder, and their very brilliancy but dazzles to blind, and leaves us still wandering in darkness and in gloom groping after truth, if happily we may find it. Let us try and find out, if possible, the secret of the power it exerts over so many millions of mankind; this lies not in its effects upon the stomach in aiding or assisting the process of digestion, nor in any tonic or strengthening influence it may exert upon the system generally. It is the change it produces on the nervous system in which the magic of its power lies, and which renders its potency for good or for evil so enormous.

I remember in my boyhood of a neighbour who, though a little man, had at all times a knowledge of his own importance, but on market-days, on his return home, that sense of his importance had been considerably increased; he would strut up and down, stating that he was cock-of-the-walk, and woe betide any unlucky individual who dared to dispute his statement. I suppose to have a very exalted idea of one's self may be pleasant, and, perhaps, beneficial, but it rendered him very ridiculous in the eyes of all his acquaintances. I knew another who, when struggling with difficulties, and when a bill was falling due which he found it difficult to meet, he would go home, get a bottle of whiskey, and go to bed; he was enabled thus to forget all about his bill, though for the time being he was as rich as Rothschild, built no end of castles in the air, and only awoke from his pleasant hallucination to find the bill protested, his credit gone, and his commercial ruin complete. That it can impart a glow of pleasure and of comfort; that it possesses the power in some temperaments of inducing oblivion of depressing circumstances, and evolving those of a pleasing and exciting character; that it imparts courage and energy, enabling some individuals to do what otherwise they would not, could not, and dare not do, cannot, I think, be disputed. If you can define anything to be beneficial which adds to the pleasure and well-being of an individual, without any counteracting effects, either immediate or remote, then I cannot conceive how any one can deny that thousands are

daily enjoying a pleasure, and, consequently, deriving a benefit from consuming a variety of drinks which contain alcohol. Whence then the terrible effects of drunkenness which lie around us? There are constitutions to which a small dose (how small I am not prepared to say) acts as a spark does to gunpowder, and leads to explosion. They cannot drink without getting drunk. I meet many of this type; they continue perfectly sober for months, perhaps without tasting a drop, but directly they take a glass, it is all up with them, and they never stop till the prison-cell arrest them, or sink into some other pit, till they can sink no lower.

Another class drink moderately daily, but take periodical attacks of drunkenness in which they commit the most disgraceful excesses, causing desolation and ruin if engaged in business, and bringing shame and disgrace and sorrow on their families. They awake with the most dreadful horror and bitter remorse, but only to repeat, after a time, the same sad cycle of events. I have known a few of whom it could not be said they were sober for years. They were constantly drinking, and the system thoroughly saturated with it. Even in such a state a considerable degree of intelligence, and even of commercial sharpness, remained. In these individuals the nervous system seemed to possess a marvellous power of resistance, and the destruction which inevitably comes, comes through the vascular system and secreting glands.

A most important question arises—are we in any way, however, unconsciously or unwillingly contributing to swell the list of drunkards? I have often heard this charge brought against medical men, and have hitherto set it down to that tendency to make excuse and throw the blame on others which has existed ever since the first sin was committed, and the first excuse offered. “The woman she gave me, and I did eat;” but recently cases have come under my notice where I cannot doubt the truth of the assertion, that the medical directions with regard to stimulants were the first step in a downward career to ruin, little dreamt of by those who prescribed them.

I look upon anæsthetics as one of the greatest boons that has ever been conferred upon suffering humanity, enabling us to wrap a patient in a gentle slumber, and carry him through the most terrible moments of existence without disturbing his tranquil sleep. These benefits have been dearly paid for, and many a valuable life has been the forfeit, but the benefits derived have been worth them all. Is it so with the administration of stimulants; do we derive benefits and advantages at all equivalent to the evils which follow, and to the risk and danger in which we place our patients. Have we any means of distinguishing beforehand the peculiar idiosyncrasy of an individual, that lays him at the feet of his passions, when once alcohol has been prescribed for him—or can it only be discovered when the fatal step has been taken, and when the mischief is past recall?

I thought it would contribute materially to our knowledge on this point to procure, from one of intelligence and education, an account of the steps by which she was led to abandon home, and friends, and comforts, and all that we hold dear in life, for the spell and the charm that circles round the "cup:"—

"DEAR SIR,—Agreeable to your request, I will try to state how the degrading besetment I am now reaping the reward of first became a habit. I feel recalling these reminiscences rather more painful than I anticipated, and would gladly give it up but that I prayerfully hope it may benefit some, and also help myself to persevere in my determination never again to be the slave of such a vice—but to the point. Up to the age of forty I had been a Sabbath school teacher, a tract distributor, and, as I thought, a Christian. Then came the testing-point. My dear father was taken from us; we had to leave the house that had been our home in infancy, and, although left in comfortable circumstances, I could not from the heart say, 'Thy will be done.' I had a serious illness, reduced almost to a skeleton; the doctor, when I was recovering, ordered me port wine; I took it for a short time, but it produced such an unpleasant hot sensation in the head and face that I was advised to try a tablespoonful of brandy in water. This I soon tired of, as I always associated the taste of brandy with castor oil. Would to God it had ended there; but a dear good old lady, a friend of ours, told me to substitute whiskey—that was the fatal step. At first I thought it nauseous, but imperceptibly I began to wish it was the time to take it. I would not acknowledge this even to myself for a time, but gradually I began to increase the quantity, then to take it oftener, flattering myself that I needed it for some ailment or other. Oh! the sophistries of the human heart, when it wants to cherish any darling sin. After I had given way, and taken the subtle poison to excess, I persuaded myself it was all for the best, for had I not taken it at that time I should certainly have lost my reason, and so things went on growing worse and worse. Never can I forget the first day I knew it had been noticed on me, and commented on. I tremble now when I recall the feeling of degradation and self-reproach, and how all the meanness and deception I had hitherto used to obtain it rushed to my mind, overwhelming me; but all this, instead of deterring me from ever looking at it again, only urged me on to get more to stifle the bitter anguish. Will you believe me, sir, when I tell you that I loathe the sin of drunkenness more than any other, and did at the time I was taking it? Mine has been one of the worst cases; I could not think I was taking it as a stimulant to elevate, but as a necessary sedative to quiet the irritation of the nerves. Oh! when I look back, I wonder how I could so blindly deceive myself, and now feel, while writing this, what if it should ever occur again. You who have never known its power can never tell or know what it is to be its slave—better, far

better, a thousand deaths—but oh! I must claim deep, deep sympathy for all its victims.”

I thought the above statement from one sensitive, educated, and refined, would give us a deeper impression of the dangers that may hang around our directions than any words of mine could do. I trust that from this discussion some prophylactic measures may be devised, and some remedial ones suggested for that large and unhappy class who are suffering from one of the most hopeless of maladies. If there be a gushing wound that calls for staunching—if there is a bitter wail of anguish and of agony that ought to be heard—if there be an imploring prayer for mercy and for pity that should be listened to, it is ascending from those who have loved ones and dear ones that they would give their heart's blood to redeem from the most degrading of bondages, but they are helpless without your aid. Are they to be told that liberty is too sacred to be interfered with, and freedom an inalienable right? The only reply that can be given to such a statement is—O liberty, freedom, religion, what wrongs are being done in thy name.

The discussion was then adjourned.

HYDROPHOBIA AND RABIES.

THE Committee recently appointed by the Scientific Grants Committee of the British Medical Association “to organise an inquiry into the Causation, Pathology, and Treatment of Rabies and Hydrophobia,” consisting of Mr. Callender, F.R.S., Dr. Burdon Sanderson, F.R.S., Dr. T. Lauder Brunton, F.R.S., Mr. Ernest Hart, and Dr. Gowers, desire to announce that they will feel favoured if any medical gentleman having under his care a case of hydrophobia will kindly communicate with them. In any cases of hydrophobia or of rabies in which a *post mortem* examination is made, they will be glad to receive for investigation the following parts—namely, the spinal cord, medulla oblongata and pons Varolii, a small piece of the cerebellum, corpus striatum, convolutions, one of the salivary glands, the nerves leading to the part bitten, and the scar. These should be at once placed in a mixture of equal parts spirit and water (or, if the organs be at all softened by commencing decomposition, in a mixture of three parts of spirit to two parts of water), and forwarded with as little delay as possible, together with a report of the *post mortem* appearances, to the Office of the British Medical Association, 36, Great Queen-street, London, W.C. The members of the Committee are also anxious to have the opportunity of visiting cases of hydrophobia under treatment, or of attending any *post mortem* examinations in fatal cases.—*British Medical Journal*.

CLINICAL RECORDS.

Compound Fracture of the Left Radius with Dislocation of the Ulna ; Removal of half an inch of the Radius by the Antiseptic Method ; Perfect Recovery without Suppuration. By SURGEON-MAJOR J. H. PORTER, Assistant Professor of Military Surgery, Army Medical School, Netley.

J. W., aged six and a half years, a strong, healthy, intelligent little fellow, was admitted into the surgical division, Netley Hospital, on the morning of the 15th September, 1877 ; he was then in a state bordering on collapse, having fallen from a verandah twelve feet from the ground ; I saw him about five minutes after the accident, when he was suffering from a contusion on the head, and a compound fracture of the left radius about an inch and a half from the wrist-joint. The bone protruded for about half an inch on the anterior of the forearm, and towards the ulna. The ulna was dislocated, forwards and inwards on the side of the carpus, and the hand itself bent back on the forearm by the force of the extensors.

The wound had been dressed with carbolised oil and lint almost immediately he was raised from the ground, and had not been in any way soiled.

As soon as the boy had recovered from the shock he was placed under the influence of chloroform, and the wound being brought under carbolised spray, reduction of the fracture and dislocation was attempted, but this was found impossible, as the fracture was in an oblique direction and very irregular. The wound was then enlarged for about half an inch above and below, and a further attempt made at reduction, which being unsuccessful I removed with a straight bone-cutting forceps the protruded portion of the bone, amounting to half an inch. The ulna was then placed in its normal position, and, there being no hæmorrhage, the wound was at once dressed with the usual carbolised dressing, consisting of oiled silk Mackintosh, and numerous folds of gauze. A splint was placed along the back of the hand and forearm, and secured with a light plaster-of-Paris bandage, the thumb and fingers being left free. The bandage was so arranged that when dry a window was formed in it for observing the condition of wound and applying dressings.

On the following day the bandage in the immediate neighbourhood of the wound was slightly stained from oozing of blood, but was perfectly dry, and there was no pain, high temperature, or the slightest constitutional disturbance.

On the 24th September, or ninth day after the accident, the dressings

were removed under the carbolised spray, the oiled silk was found damp from a slight serous looking discharge, but there was no pus or unpleasant smell, and the wound had almost healed. The wrist was, however, considerably swollen and painful, the hand and fingers being very stiff.

On the 28th the swelling of the wrist had subsided, which caused the bandage to become loose; it was therefore removed with the usual antiseptic precautions, and another applied, but the boy was allowed full use of his fingers, over which he had slight power, and which I forcibly flexed and extended.

October 9th.—The dressings were again removed, but the wound was perfectly dry and healed. A short splint was now secured by an ordinary bandage to the inside of the arm, so as to merely support the wrist and seat of fracture, and allow free motion of the hand and fingers.

October 15th, or one month from date of accident, the wrist and seat of fracture still considerably swollen, and power of pronation and supination impaired; commenced exercises to secure these motions.

November 5th.—The boy is now able to dispense with the splint, and can lift a considerable weight without inconvenience. The power of flexion and extension of the hand and fingers, and pronation and supination of the forearm are unimpaired.

On the 15th November he was discharged from hospital, being able to do as much with the injured arm as with the sound one. There was some thickening about the seat of injury, which rendered the lower third of the forearm not quite so symmetrical as the other.

DR. STEEVENS' HOSPITAL.—*Complete Suppurative Destruction of Lung after Pneumonia.* Under the care of DR. GRIMSHAW. Reported by JOSEPH A. BEATTIE, Clinical Registrar.

THE following case presents some points of considerable interest, the almost complete destruction of a lung, as a sequence of pneumonia, being sufficiently uncommon to deserve a special record:—

Bridget L., aged twenty-five, domestic servant, was admitted into No. 6 ward, Steevens' Hospital, on 22nd September, 1877. Family history fairly healthy; parents both dead; the mother died of a chest affection, at fifty years of age. The patient was a twin, the other child being still-born. She states that she has always enjoyed good health, and, although obliged to work hard as a thorough servant, she had seldom occasion to complain on the score of illness.

Two months before her death she caught a heavy cold from wet feet whilst on an errand, the symptoms commencing with a pain in her back, which was of such severity that she was obliged to be carried home. Shortly afterwards she was troubled with a harsh cough and sense of constriction across her chest, which became so very aggravated in character

that she could not breathe except with considerable difficulty, and now, being seized with pains in chest and side, and cramps in her lower limbs, she was admitted into hospital on above date. On admission she had been apparently ill for some days, and stated she had been unable to sit up in consequence of the pain. A physical examination discovered complete dulness and great percussion resistance over the lower part of the right lung, increased vocal resonance, amounting almost to pectoriloquy at some points; some fine crepitus was audible under the angle of the scapula. The dulness did not alter with position. The expectoration was of a thick tenacious character, yellowish in colour, but without any rusty streaks. The patient, however, stated that she had expectorated some "dirty red stuff." The pulse was quick, and the temperature varied between 101° and 103° for a week, and then sank to normal in the morning, rising to 100° or 101° , in the evening. Large doses of quinine were ordered, under which treatment her general condition steadily improved, but the lung affection for three weeks remained almost stationary. Now a coarse crepitus began to develop itself at the base of the right lung, gradually appearing in patches elsewhere; the dulness gradually extended, so as to occupy the whole of the left side. The patient now began to expectorate sanious matter, not in small quantities at a time, as the pus had been previously expectorated, but in considerable quantities, at intervals of from one to three days. On some occasions so much as a pint of this sanious fluid was expectorated in the course of an hour or two. The patient now began visibly to decline, and a slight icteric tinge appeared on the skin; diarrhoea set in, and the liver became enlarged. Ten days before death she complained of intense pain in the right side; referred chiefly to the site of the 4th, 5th, and 6th ribs in the axillary line. This pain had the character of a pleuritic stitch, but no friction sound could be heard over the point complained of. In three days from the first complaint of pain a tumour began to develop itself at this point, extending posteriorly towards the angle of the scapula. The tumour continued to enlarge, and manifestly contained fluid communicating with the interior of the chest. After careful consideration the opinion was arrived at that the tumour contained pus, and was in communication with the deposit of matter within the chest, from whence the purulent expectoration had come. Under these circumstances, and considering the extremely feeble state of the patient, it was considered inadvisable to resort to any surgical interference. The patient gradually became weaker, and died on 23rd of November. The *post mortem* revealed the following state of affairs:—

Body.—Slight, but fairly nourished. The anterior aspect of abdomen, thighs, and right ankle present well marked livid patches. Subcutaneous adipose tissue almost absent.

Abdomen.—Great omentum, normal; mesenteric glands, indurated and

enlarged; pancreas, normal; small intestines, externally normal (not opened); large intestine, the coats of the cæcum and ascending colon much thickened; the surface corresponding to the ileo-cæcal and ileo-colic valves was found to be ulcerated through to the serous membrane. On slitting up cæcum and ascending colon, a large number of ulcers were found studding the inflamed mucous surface, being most numerous on the ileo-cæcal valves; the remaining portion of the intestinal tract was not opened. The stomach was quite empty, and contracted to size of transverse colon; spleen, normal; kidneys and suprarenal capsules, normal; pancreas, normal; liver, not weighed, but containing one very large abscess, communicating with the lung through the diaphragm, and two or more smaller abscesses, and having on its anterior surface a distinct and extensive cicatrix.

Thorax.—On opening right side of the chest a large quantity of fluid, at first of a sanious character and subsequently of a thick and purulent nature, escaped. The left lung was nearly normal, slight hypostatic congestion and traces of compensatory emphysema. The substance of the right lung was completely disintegrated, save a small portion attached to root measuring $\frac{1}{4}$ inch thick and 3 inches long. The upper surface of diaphragm was found perforated, and communicating with a large abscess in the liver. The ribs of this side were denuded of their pleural investment, being rough and carious to feel, and the intercostal muscles between 5th and 6th ribs were destroyed, admitting communication with a large abscess situated below posterior inferior angle of scapula.

Remarks.—Dr. Grimshaw remarked that he considered the case to be one of suppurative destruction of the lung, and that while he expected to find extensive changes produced by the suppuration, he did not anticipate such a complete destruction of the lung, the pleura, and a portion of the liver, as was discovered after death. He considered that the case was one of a gigantic abscess of the lung, involving in a secondary manner the walls of the chest, the diaphragm, and liver. The progressive nature of the physical signs pointed to gradual extension of the suppurative action from the base to all other parts of the lung, the communication through the diaphragm and intercostal space having probably followed the course of pleural adhesion which had taken place at the commencement of the pneumonic attack. At no time was there any sign of free fluid in the pleura. The small and ragged portion of the lung remaining pointed to progressive suppurative stages and contraindicated collapse. He considered the case to be one of extreme rarity, and could not recollect having read of any similar one.

Notes on Cases of Amputation through the Shoulder-joint. By Mr. E. STAMER O'GRADY, M.R.I.A.; Ch.M., M.B., A.B., Univ. Dub.; Surgeon to Mercer's Hospital; Fellow and Member of the Surgical Court of Examiners, R.C.S.; Licentiate, King and Queen's College of Physicians; formerly Lecturer on Surgical Anatomy at the Carmichael School of Medicine, &c.

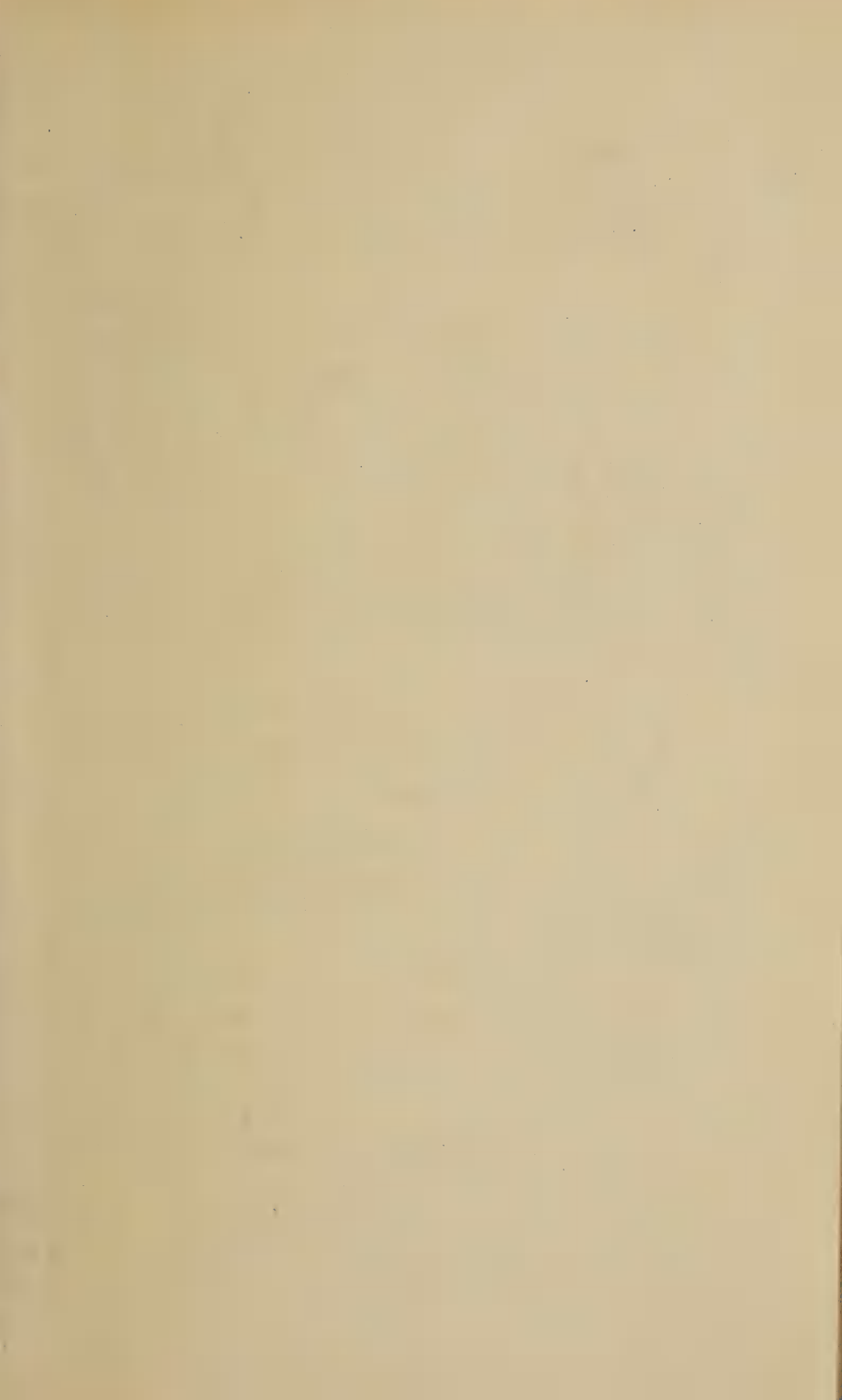
CASE I.—*Disarticulation of Shoulder-joint for Malignant Disease of Humerus.*—T. M. (No. 256), a pensioner, aged thirty-five. This man had led an irregular life, but had married young and never had syphilis. In India he suffered severely from “liver,” intermittent fever, and dysentery. Marks of extensive former “cross-cupping” are over the abdomen; the spleen much enlarged; the size of this organ varied, he said, considerably from time to time. He also, some time previously, had purulent pleuritis, the sequence of a stab received in a row with a comrade, and eventuating in 84 ozs. of pus being drawn off by tapping. The general aspect of the patient is in a marked degree leucocythæmic. He, however, considers himself, notwithstanding, to be a strong healthy man. Some five months previously to his applying at Mercer's the left humerus, immediately above the elbow, began to be the seat of severe pain, and was soon after noticed commencing to swell, the growth being attended with increasing suffering, which soon became, and continued to be, intense. There was also œdema of the forearm. Relief had been unavailingly sought in different hospitals where various means, both local and constitutional, were tried without any benefit. Latterly the affection was looked on as malignant. When he came under my observation the lower end of the humerus was greatly enlarged, being in size fully equal to that of a large orange or Spanish onion; the bone enlargement gradually tapering off above, and being recognisable even beyond the insertion of the deltoid; it was the seat of severe and unintermitting pain. Two much enlarged, though freely movable, and not indurated glands, were discernible in the axilla. The heart sounds were healthy, and the urine free from albumen.

The patient's sufferings were very urgent, and a few days after his admission the limb was amputated^a through the shoulder-joint; pressure on the subclavian by a padded key, satisfactorily restrained the hæmorrhage, but little blood being lost. The limb separated, the axillary artery was secured by acupressure after the “first method,” the other vessels being all twisted except one, which could only be staunched by ligature. The surface of the wound having been well swabbed over with

^a Examination of the bone on section was confirmatory as to the diagnosis of the malignant nature of the affection. Unfortunately, a carefully prepared report obtained at the time as to the pathological appearances, microscopic and otherwise, has been mislaid. The two enlarged glands were removed at the same time as the limb.

a forty-grain solution of chloride of zinc, was brought together by twisted sutures, except at the most depending part, which was left open for escape of the secretions, and from which pink-coloured fluid soon began to ooze in considerable quantity.

The after-progress of the case may be briefly summarised:—The operation was well rallied from; the patient, relieved of his long-standing suffering, slept as it were through the first few days, scarce awakening otherwise than for the purposes of taking nourishment, or when called on by the requirements of the bladder. At first the needle was seen to be so powerfully throbbled against by the acupressed vessel as to cause anxiety. This condition lasted for about forty hours after the operation, and was accompanied by a feeling of pulsation on the surface of the stump, which continued after this period, and was plainly discernible when the acupressure was removed at the termination of the third day. Its withdrawal was unattended by any bleeding—a considerable portion of the wound was already united; most of the suture pins (which had an annoying tendency to hitch and become entangled in the dressings and bed-clothes) were also now taken away. On the fifth day there was considerable febrile disturbance, this had somewhat suddenly become developed; the pulse which, except on the first night, when it reached 104, had fluctuated between 90 and 98, now ran up to 140, with hot, though perspiring skin, tympanitic abdomen, and tendency to nausea. The stump was much swollen, and it was observed that the portion left open as a vent for the exit of secreted fluids had closed subsequent to the extraction—on the third day—of the mesh, saturated in carbolised oil, inserted at the time of the operation to keep the opening patent. The newly-formed adhesions here were ruptured, when about ten ounces of stinking sanguinolent fluid escaped with a gush. A fresh mesh having been inserted so as to ensure the drainage, soft pads were secured over the covering flap, to keep it steadied against the deeper parts; the stump was then poulticed, and a brisk purgative enema given. In a few hours the alarming symptoms had entirely disappeared, and thereafter amendment rapidly progressed. The patient was able to sit up on the tenth day; in three weeks the wound had virtually healed, and in a month he took his discharge in excellent health, and able at once to resume his ordinary duties as one of the City Commissionaires. With the exception of occasional opiates and enemata, there was no medicinal treatment whatever during the progress of this case. Its publication has been held over till the sequel should be seen. On various occasions subsequent to the amputation the man, from time to time, came under treatment for attacks of liver trouble, delirium tremens, dyspepsia, and later on he laboured under a peculiar purpuric rash of the lower extremities to which no definite name could be given by any of the many practitioners under whose care he successively placed himself. He was not in Mercer's for



Platell



Platell



several months prior to his decease, but it was ascertained that he eventually died in another institution rather more than five and a-half years subsequent to the amputation. The physician, under whose care he then was, informed me that, in his opinion, death was caused by malignant disease in the abdomen, and implicating the stomach. There was no autopsy.

The photograph from which Plate No. I. is copied was taken more than four years after the operation. This illustration contrasts with the others in showing the remarkable change in appearance shoulder stumps undergo after time has effected the absorption of the superfluous portions. Whilst this man's stump was a fresh one it was very similar in appearance to that figured on the right hand side of the Plate, and later on resembled that of the girl's in the stage depicted in Plate No. 3.

CASE II.—*Disarticulation of Shoulder-joint for Malignant Affection (Warty-ulcer of Cicatrices), occurring in Cicatrix thirty-five years' after Severe Burns.*—Wm. B. (No. 1,365), a healthy-looking man, forty years of age, a dyer by trade, had been extensively and severely burnt thirty-five years previously, over the right upper extremity, and side of the body, as a result of which the arm was bound down by adhesions, and the elbow permanently flexed to a right angle by the cicatricial bands and contractions. The repaired parts had remained sound till about a year ago, when an ulcer formed on the inner side of the elbow and arm. This was excised at another hospital. The relief thus afforded was but very transitory, and when the case came under observation at Mercer's there was on the same site a recurrent ulcer about as large as the palmar surface of a man's thumb, and which exhibited in a marked degree the appearances characteristic of the affection so intimately associated with Marjolin's name. It was ascertained that prolonged topical and constitutional treatment, including a steady course of arsenic, had been already tried. In the local condition of the parts further temporising measures were inadmissible, and the patient, refusing amputation, left the hospital. In somewhat more than three months he returned, now greatly altered for the worse. An Englishman by birth, he had in the interim been over to his native country, and tried various hospitals, then, finally having determined on getting the limb removed, he came back to Mercer's for the purpose of having the operation done. During his peregrinations he said he had suffered severely from fatigue, privation, and otherwise. The man's appearance fully confirmed his statement. In those three months the ulcerated surface had spread greatly; it now exceeded in size the entire palmar surface of a large hand, whilst an abscess on the ulnar aspect of the forearm, and about to burst, threatened speedily to add to it considerably. The extent and resisting nature of the firm fibrous bands which bound the arm down to the side, effectually prevented any

conclusion being arrived at as to the presence or otherwise of glandular contamination.

After a short interval of rest, during which he was well fed, the limb was amputated through the shoulder-joint; in the course of the procedure, notwithstanding that the subclavian artery was well commanded with the padded key, by my then colleague, the late Professor Morgan, some six ounces of blood were unavoidably lost. There was tendency to free hæmorrhage at the posterior and upper portion of the incision; this was only to be controlled by local digital pressure. As the knife traversed the parts in the formation of the inner or axillary flap, it split more or less longitudinally several enlarged regional glands; these were about the bulk of an ordinary bean and about twice as long. After the vessels, nine in number, were secured with carbolised catgut, these wounded, and some other enlarged glands were excised. The usual forty-grain solution of chloride of zinc was then thoroughly applied to the wound, which was kept open for over four hours till all tendency to hæmorrhagic oozing ceased, when its edges were coapted (provision being made for free drainage by a linen mesh, soaked in carbolic oil; this was lessened in twenty-four, and removed in forty-eight hours; it effectually secured ample vent for all fluids both at the time and subsequently) by points of carbolised catgut, and the parts further supported by pads and straps of adhesive plaister. The patient, who at the time required the liberal use of stimulants, rallied well from the operation, suffering however a good deal from local pain, for which opiates had at first to be freely given. In the afternoon there was some reactionary hæmorrhage; this yielded to the local application of bladders containing broken ice. Progress to recovery was, on the whole, satisfactory. The catheter required to be used at regular intervals for three days. The operation was succeeded by considerable constitutional disturbance, which lasted longer than usual; in the after-treatment anodynes had to be used freely, and stimulants moderately, for some time; sustained tonic treatment was also necessary; as a sedative, bromide of potash was found specially useful. Food was well taken throughout. As local applications, poultices, with varied stimulant dressings along the lips of the wound, were found during the earlier periods to agree best. On the seventh day there was slight hæmorrhage to the extent of about one ounce, when it ceased spontaneously, though, for the succeeding two days, there was some disposition to ooze small quantities of sanguineous fluid occasionally. A mixture containing liquid extract of ergot, muriate of tincture of iron, with tincture of digitalis, was now taken in addition to quinine pills. Some cardiac excitement with high pulse continued for many days. As far as practicable the patient was kept in the open air on a bedded stretcher. Probably a good deal of the more unfavourable symptoms at this period were due to mental trouble, in connexion with some domestic occurrences,

the intelligence of which worried him greatly. In a fortnight after the amputation, he was able to get up and walk to the exercise ground. His health now began to improve apace, though the efforts at local repair continued very languid, and required constant change of stimulant dressings. Fifty-six days after the operation he was sent to a convalescent institution, in good health, and with the stump nearly healed, the soft part however being in a state of brawny indurated thickening. A few weeks later, he returned to England, now in excellent health, quite free from pain or suffering of any kind, and the tissues of the stump steadily losing their unfavourable characters, and becoming healthy. W. B. has since occasionally written as to his state; his last communication, just three years after the amputation, stated that he was in perfect health, and had no indication of re-appearance of his trouble. With the progress of time the stump lost its indurated and thickened condition, becoming fairly supple, pliable, and considerably atrophied. The photograph from which Plate II. is copied, was taken immediately before his departure to England, whilst the stump was still full and tumid. In consequence of the retraction and diminished tension of the thoracic parietes, the cicatrices here situated had become greatly modified in appearance, and scarcely show at all in the photograph, which, in this respect, presents a notable contrast to one taken before operation, in view of preserving the pathological appearances.

CASE III.—*Disarticulation of Shoulder-joint for progressive Necrosis of Humerus, the Elbow-joint on same side having been previously excised.*—M. B. (No. 1,758), aged eighteen. There is an obscure history of "hurt" to the left elbow a couple of years previously, whilst engaged in some avocation as a country farm-servant. Since that time disease of the joint has been gradually developing itself, becoming worse and worse, and latterly the cause of much pain, attended also with marked depreciation of general health. There had been catamenial suppression for eight months. The limb was useless; much deformed; extended and immovable; the region of the elbow swollen, of an irregular ovoid form; some very tortuous sinuses (examination of which, however, by the probe gave no information) discharging large quantities of foetid pus. The joint was excised by the single straight posterior incision; this traversed a thickness of more than two inches of much diseased soft parts before reaching the bones. As these were exposed it was seen that a marked displacement existed, the head of the humerus lying jammed in between the bones of the forearm—the external condyle looking forwards; in this position the bones were firmly fixed, which added to the difficulties entailed by the thickness of the soft parts in getting at them to apply the saw. The articular ends of all the bones, the humerus being much the most affected, were necrosed, and required extensive removal; considerable

quantities of unhealthy soft tissues were also clipped away. Altogether the case seemed one in which amputation should be performed, and in anticipation of it proving to be one in which it would be necessary to proceed to this extremity, the girl's consent to the measure had been sought, but was unconditionally refused; her determination was, under any circumstances, to take her chance of excision. The ulnar nerve was not seen at all during the operation. The unkind and unyielding nature of the soft parts prevented the satisfactory disposition of the bones, which could not be brought into anything like desirable propinquity.^b The limb was "put up" on an anterior gutter splint, suitably padded; the instrument was "for the present" fixed about midway between straight and a right angle; its degree of flexion could be altered at will by the lateral thumb screw. The violent and uncontrollable temper of the patient, who, in oft-recurring hysterical paroxysms, could only with difficulty be restrained from deranging and even attempting to remove the apparatus, much embarrassed the earlier after-treatment. Constitutionally there was soon marked evidence of relief, and locally too, for some time, the resection wound looked extremely well, in ten days having contracted satisfactorily, and being filled with healthy granulations. The limb had been gotten more flexed, and the girl was now able to go about. This hopeful state continued but a few days, when matters again took a retrograde course; the discharge from the wound became increasingly unhealthy, and flowed in larger quantity. There was an eminently hysterical condition, associated with a well-marked phantom tumour, as large as a full-sized cocoa nut, at the bottom of the belly; still the operation wound slowly closed, and was in three months healed, except two sinuses which gave exit to a large quantity of unhealthy pus daily. No attempt at osseous consolidation had taken place; the region of the elbow remained quite flail-like. Pretty much in "*statu quo*," M. B. went back to the country nearly five months after the excision. She remained there almost five months more, under the care of the gentleman to whose kindly remembrance of student days I had been indebted for this most interesting case. When she returned to Dublin she was much improved in health, and the catamenia had become regular; the affected limb, too, was changed for the better; though the sinuses still continued to discharge ill-conditioned pus, it was in much diminished quantity; the soft parts had become pliable and healthy, and the divided bones drawn into approximate apposition. There did not seem now to be much active bone mischief. Under these circumstances, at the girl's earnest

* Subsequent measurement showed the length of each bone removed to be—humerus, in three successive portions, four and a half inches; ulna, two three-quarters; radius, one and a quarter; this bone being cut obliquely so as to save the insertion of the biceps. The operation was done subperiosteally.

^b *Vide* report of this case further on.

solicitation, it was determined to make another effort to save her limb. To meet this view the ends of the bones were again exposed, and their terminations sawn off. The two bones at the forearm had become fused together, so that a single (and healthy) even bone section was now obtained for application to the newly divided humerus after removal at its necrosed end, which was surrounded by unhealthy involucrum. The bone sections were so made as to admit of the limb being put up at a right angle. A little beyond their fresh sawn extremities the bones of upper and lower arm were pierced with a small drill, and the bone faces, *which readily came into satisfactory apposition*, were so retained by a strong carbolised catgut interosseous suture. The wound was closed, adequate provision being made for drainage, and the arm put up in a suitable gutter splint, in the flexed position.

As an immediate sequence of the operation there was more local pain on this than on the former occasion. For a time the case now seemed to progress fairly favourable; the splint was not stirred till the nineteenth day, when, though there was no consolidation, the ends of the bones were in contact, and the general appearance of the wound was not wholly unpromising. Soon, however, the health began to give way again, and the state of the arm to grow daily worse—disease evidently spreading up the humerus; matters proceeded from bad to worse; fresh sinuses formed further up the limb, from them copious fœtid discharge ran off in large quantities, reducing the girl to a low ebb before she consented to the amputation, which was done just thirteen months after the original excision, and nearly three after the second operation. After raising the deltoid flap it was necessary to seize some spouting vessels in Langenbeck's hæmostatic forceps—one of them being of very large size was ligated at once. As the knife progressed along the humerus to form the inner flap, other vessels sprung; but by supplementing the key pressure on the subclavian, by following the knife with the left thumb—the fingers lying along the integument on the inner surface of the arm, and thus immediately compressing the parts^a—the total of blood lost was inconsiderable. The limb severed, the more prominent vessels were seized in another Langenbeck's forceps, and the ligation proceeded with *seriatim*; in all eleven vessels were tied with carbolised catgut. As in the preceding case this wound was treated with solution of chloride of zinc, kept open some hours, and then dressed in a similar way. The patient now responded well to nutritious feeding and tonic treatment. Her health rapidly picked up, and in less than a month the stump was entirely healed.

CASE IV.—*Disarticulation of Shoulder-joint for Necrosis; extensive suppuration following Compound Fracture of Arm.*—E. M. (No. 1,550), aged twenty-two. A labourer engaged in building-work was admitted for

^a The entire remnant of the humerus was found to be dead.

injuries received a few minutes previously, by falling off a scaffold from a height of about twenty-five feet. There were severe contusions of right side of face and at the region of the right hip, as well as a bad lacerated and incised wound at back, below angle of scapula, with lesser injuries further forwards of the soft parts covering the same side of the thorax. The right humerus was badly broken in its lower half, causing much deformity of the limb. From a wound in front of the elbow the blood flowed freely—the entire arm was extensively emphysematous. According to the customary mode of treating compound fractures at Mercer's, and which is usually found there to be attended with excellent results, the wound was "sealed" by a bit of lint soaked in the blood, over this a pad saturated with Friar's balsam. The limb was placed lying in a good position on a padded rectangular splint applied to its inner aspect, with short straight splints surrounding the fracture. Notwithstanding his severe injuries the young man had remained conscious, being scarcely even "stunned" at the time of the accident. After an opiate he rested comfortably. At first all went well, but three days subsequent to admission there was a threatened attack of pneumonia, accompanied by much constitutional disturbance, prostration, and severe suffering in the injured limb. Stimulants were now freely needed. On the tenth day he was constitutionally convalescent, and by his own desire allowed to sit up. To relieve tension the bandages were opened. The limb was lying in good position, and the dressings were left undisturbed; the emphysema was well marked still, this condition being observable a week later on, when all yet appeared to be going on well. The wound under the scapula and the other injuries of the soft parts were now healed. At this juncture the patient, whose wife had been recently confined, left the hospital by his own particular desire, promising to attend as an extern. When he came back in a week (twenty-eight days after the accident) the pads which had sealed the wound were found detached, and the wound itself soundly healed; the fracture lying in excellent position, but, as yet, no apparent attempt at union. The patient, who was looking very pale and anæmic, was put on a ferruginous tonic treatment with cod-liver oil. An admission to the convalescent home was also obtained for him, but he would not stay there.

The future progress of the case was altogether downward; after being four weeks out of the hospital he sought re-admission now in wretched health, irregular lumps of ossific matter had been thrown out in the neighbourhood of the fracture, but no union whatever. There was much deep-seated inflammatory mischief present. In a few days an abscess burst, giving vent to a profuse and very fœtid discharge. With good feeding and tonic treatment it was attempted to keep up the health, but he began to sweat profusely at night, rapidly lose flesh, and suffer from cough. Abscesses formed in quick succession, till the entire region of

the shoulder was one mass of purulent deposits, with sloughs extruding from the incisions and sinuses discharging everywhere, in the axilla, front and back of arm, and through the substance of the deltoid, the integument thereabouts being much undermined and damaged. The exigencies of the case necessitated the amputation (performed a little more than two months after the occurrence of the accident) being done in an nondescript sort of fashion, the number and situation of the sinuses and ulcerated openings was such that in no way was it possible to fashion a satisfactory stump, by any of the recognised modes of operating. With the material at disposal, a modified deltoid flap seemed to promise the best covering, though what would be the centre of the flap here was so diseased that it was mapped out by an inverted Λ , the contained tissue being rejected and left for removal with the limb. In like manner, when cutting the inner flap, the knife was made to encircle and isolate for removal some very diseased tissues at the axillary margin.^a Through the splitting of the deltoid the resulting stump was, as it were, composed of three flaps. Eleven vessels were secured by carbolised catgut, and the entire surfaces of the wound swabbed well out with the forty-grain solution of chloride of zinc, the fluid being also injected freely into all practicable sinuses and recesses. Like as in the preceding cases, the wound was kept open some hours. Under the influence of a warm bed and full opiate the patient rallied well and rapidly. So great was the relief that within two hours after the operation he asked for, and thoroughly enjoyed a smoke. The wound was treated similarly to the mode already stated. Urine was passed from the first without the aid of the catheter. It is unnecessary to follow the details of the case. The operation was done in the month of June, the weather being remarkably fine, and from the second day the patient was kept the best part of the day in the open air on a bedded stretcher. The discharge at first was brown coloured, bad smelling, with ill-conditioned appearance at the wound, and attended by a good deal of constitutional disturbance. These conditions mended with a tonic and stimulant line of treatment. Charcoal and barm poultices, with camphorated spirit dressings were found to agree best with the wound, which in a week began to granulate and suppurate healthily. In a fortnight there was a wonderful restoration, cough and night sweating gone; pulse good; patient able to be up; the edges of the stump around its circumference had healed; a large broad ulcer however occupied the space corresponding to the interval of the two portions of the deltoid flap; this was kept carefully supported, and the edges approximated; it slowly healed in, the wound not being soundly cicatrised for nearly two months, though the man had for a considerable time previously regained the rudest health. As the stump healed, the two portions of the deltoid

^a The bone was black and dead, having some irregular lumps of callus thrown out at the locality of the fracture.

flap became respectively drawn in part forwards and backwards, thus lessening the depth of the Λ , and approximating its apex considerably to the axillary portion of the stump.

Plates III. and IV. represent the result in the third and present case. In the former, though taken at an early date after the operation, it will be seen that absorption has already made considerable progress towards approximating the stump to the condition shown in Plate I.; in the diseased and infiltrated structures of which the flaps in Plate IV. was formed, as likewise in the not healthy issues which had to be made use of in Plate II., this change is a much more chronic matter.

Like the other two lithographs, these plates are also from photographs, very carefully taken by Mr. Forbes, of Messrs. Robinson and Sons. The idea of placing a mirror behind to reflect the posterior aspect of the subject is borrowed from American publications.

In all four cases the plan of operation (modified as stated, to meet the exigencies of the second and fourth cases) by forming outer and inner flaps from the deltoid and axillary regions respectively was selected. The skin was first divided by a free, somewhat semicircular incision, commencing in front below and anterior to the acromion, and carried, with its convexity downwards, well back, the ideal base line being left wide to afford free scope for manipulation. During the subsequent steps of the operation a little hand pressure retracts the integuments, and then the muscular structures are cut along the outline of the incision down to the bone, the flap raised, the joint opened, and the structures proper to it with the biceps origin, and the muscular attachments to the head of the humerus (the limb being rotated, as needed, by an assistant) severed successively. The articular extremity of the bone freed, the knife was passed down along its inner side to form the axillary flap. Should the pressure by finger, or padded key handle, on the subclavian, as sometimes happens (to wit, when there is uncontrollable struggling on the part of the patient, in cases of irregular arterial distribution, and in others, where, as in some of the subjects of this communication the anastomosing circulation has been largely modified), no matter how skilfully it may be applied, not effectually control the bleeding it is an easy matter for the surgeon before cutting this flap through, by following the knife with the thumb or fingers of his second hand to grip it and thus himself ensure the silence of the principal trunk, and after the dismemberment, if need be, he can rapidly pick up, and temporarily secure with Langenbecks hæmostatic forceps any spouting vessels, prior to proceeding to ligate or twist them *seriatim*.

In its practical bearings, though the covering flap is cut with far less precision, this mode of operating is not very dissimilar from the old trapezoid flap amputation of M. La Faye. When the circumstances under which the operation is called for advisedly permit of this or other



Lithographed by John Falconer, Dublin



MR. F. S. O'GRADY. AMPUTATION THROUGH SHOULDER-JOINT.

analogous mode of operating being adopted, the surgeon in selecting such secures some tangible advantages which more than compensate for the éclat of a well executed transfixion^a operation. Its simplicity and facility of performance commend it. In common with most other amputations, where the incisions are made from skin to bone, rather than by transfixion, absolute accuracy may be secured in the dimensions and formation of the covering flap, whilst the internal or axillary flap may, by a little care, be shaped and cut with almost equal precision, the parts being so much under the control of the surgeon when it comes to be formed. The operation can be done most easily with the patient secure on the ordinary operation table, in the recumbent position, and with the arm alongside the thorax. It is indifferent whether the left or right arm has to be removed, the surgeon standing below the shoulder in the former case, above it in the latter, if he so prefer, though very many operators would find it to be equally or more handy to stand below the joint for the removal of the right limb also. A not inconsiderable advantage is that all the steps of the operation can be completed with a single knife, and no delay can well occur in consequence of its getting hitched or broken; one somewhat after the pattern of a large broad scalpel, with bellied extremity, the cutting part of the blade, measuring four and a half inches in its straight length, made of width and substance proportionate to its increased size and purpose as an amputating knife, and set in the firm diced handle of such, answers well for the entire procedure. It may be observed, too, with regard to the after-bearings of the case, that the fear of bagging and infiltration of matter along the thoracic parietes, the supposed increased liability to which has with some constituted an objection to this mode of amputating at the shoulder, seems groundless; no trouble in any of the cases arose from this source, and it would seem any tendency of the sort could be easily met and kept in check by suitable pads and bandaging. The retraction of the skin over the shoulder and thoracic parietes is apt to be very great in scapulo-humeral amputations, and after some forms of the operation requires much care and trouble to prevent exposure of the glenoid surface of the scapula or acromion.

In the first case the reappearance of malignant disease after so long an interval, and in parts unconnected by glandular association or function, was most exceptional. In this case the main vessel was acupressed, and the rest, with the exception of one tied, twisted. At that time the writer had not acquired the well-merited confidence in the use

^a Here sometimes resulting, even in the hands of experienced operators, in much bungling—the knife it may be hitching, or its point breaking, and not infrequently a deficiency of covering, the retracting integuments exposing the scapula, &c. Every teacher of operative surgery knows well that amputation through the shoulder in the living person is a widely different matter from the same procedure on the dead subject.

of carbolised catgut for securing large vessels on the faces of stumps, which subsequent and more extended experience has given him. In the other cases this agent was employed with all the vessels.

In operating on the second case special impediments, dependent on the rigid nature of the cicatricial bands and almost immovable condition of the parts, presented. The arm could not be rotated, nor any change made in its position capable of assisting the surgeon. Owing to the cicatricial and over-strained condition of the integuments the flaps were cut exceptionally long. They subsequently contracted much, and, as remarked in the record of the case, such had a marked effect on the appearance of the altered thoracic integument. Had the operation on this man been other than through the joint, there is no doubt the retraction would have rendered the covering of any other form of stump insufficient. In this case, too, the condition of the axillary glands was of great interest, as bearing on the question whether in individual cases their implication be sympathetic or specific.

It may appear that the persistent attempts at preservation of the limb in the subject of the third case was carried too far, and that so seriously shortened a limb would have been of but little service. The writer knows of one case where as much or more of the humerus was removed—at the excision of the elbow—the parts healed up, and the patient regained his health. The union was flail-like, but yet with a laced leather support all the functions of a perfectly healthy hand are available. In another case a capital limb, useful for every purpose in life remains, where diseased bone was removed from the lower end of the humerus, the head and some inches of the shaft being subsequently excised as well as the entire scapula.

Amputation through the shoulder, especially for pathological causes, is an operation of comparatively infrequent occurrence in civil practice, and cases of it will be always regarded with attention. The very numerous modifications and different modes of operating suggested and practised by different surgeons, including some of the principal leaders of our art, attest the interest with which it has always been regarded by us since it was first introduced to the profession.^a In addition to the four “clinical records” above briefly sketched, one other case of this operation, which also eventuated successfully, has occurred in my practice. Here the scapula also was removed with the limb; but as it is proposed to make it, together with other cases of shoulder and scapula excision, the subject of an independent communication, it need not be here further alluded to.

^a Whilst Le Dran seems most generally accredited with having been the first to perform scapulo-humeral disarticulation, some assign priority to Laroque, and others to Morand the elder.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

*Of Eight Large Towns in Ireland, for Four Weeks ending Saturday,
December 1, 1877.*

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES								Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea		
Dublin, -	314,666	697	662	1	49	10	4	13	16	5	27·4	
Belfast, -	182,082	441	358	—	8	21	—	—	20	7	25·5	
Cork, -	91,965	190	259	—	47	1	—	—	9	6	36·8	
Limerick, -	44,209	83	87	—	—	5	—	—	4	3	25·5	
Derry, -	30,884	77	43	—	—	2	—	—	1	—	18·0	
Waterford, -	30,626	45	65	—	3	1	—	—	3	3	27·5	
Galway, -	19,692	34	33	—	—	1	—	—	2	—	21·5	
Sligo, -	17,285	24	25	—	—	—	—	—	1	—	18·8	

Remarks.

The winter rise in the mortality of all the towns is well marked. The very high death-rate in Cork is, in some measure, due to a severe and fatal epidemic of measles, which began towards the end of October. Waterford and Dublin also show an unfavourable state of the public health. The death-rate of the period was 22·3 per 1,000 of the population annually in London, 20·2 per 1,000 in Edinburgh, and 22·3 per 1,000 in Glasgow. Omitting the deaths of persons admitted into public institutions from localities outside the registration district, the death-rate in Dublin was still as high as 26·7 per 1,000. The deaths from zymotics were 129, compared with 135, 110, 91, and 105 in the immediately preceding periods. The average number in the corresponding period of the previous ten years was 139·7. Scarlatina was less fatal, measles was as fatal as in the preceding period, and the deaths from whooping-cough showed a considerable increase. Of the 16 fever-deaths, 3 were due to typhus, 9 to enteric, and 4 to simple continued fever. Measles and fever were very fatal in Cork. Respiratory affections were again

very destructive to life in Dublin. They caused 140 deaths, compared with an average of 119·4 in the preceding ten years. Bronchitis caused 96 deaths (average = 88·8), and pneumonia 32 deaths (average = 15·7). Pneumonia was, therefore, more than twice as fatal as in the ten years. The death from small-pox was of a boy, aged six years, *unvaccinated*, who was admitted to Cork-street (Fever) Hospital from No. 3 South City District. Small-pox, unfortunately, has increased very much in London during the past few weeks. It caused 97 deaths in the present period, against 45 in the preceding four weeks. In Dublin the epidemic of measles was especially severe in No. 4 South City District, which previously had, to a large extent, escaped its ravages. Of the 49 deaths from measles registered, 20 occurred in this district.

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of November, 1877.

Mean Height of Barometer,	-	-	-	29·543 inches.
Maximal Height of Barometer (on 1st at 9 p.m.),	-	-	-	30·439 „
Minimal Height of Barometer (on 11th at 2 p.m.),	-	-	-	28·303 „
Mean Dry-bulb Temperature,	-	-	-	45·1°
Mean Wet-bulb Temperature,	-	-	-	42·8°
Mean Dew-point Temperature,	-	-	-	40·3°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·250 inch.
Mean Humidity,	-	-	-	84·0 per cent.
Highest Temperature in Shade (on 15th),	-	-	-	60·4°
Lowest Temperature in Shade (on 24th),	-	-	-	32·9°
Lowest Temperature on Grass (Radiation) (on 24th)	-	-	-	27·7°
Mean Amount of Cloud,	-	-	-	54·8 per cent.
Rainfall (on 22 days),	-	-	-	2·438 inches.
General Direction of Wind,	-	-	-	S.W. and W.

Remarks.

The atmosphere in North-Western Europe remained in a very disturbed state throughout the month, which was remarkable for the number and intensity of the barometrical depressions travelling north-eastwards or eastwards across the British Islands, with their accompanying gales and rains. On the 11th the deepest depression observed this year advanced from S.W. By 2 p.m. the barometer had receded to 28·303 inches in Dublin, where rain fell heavily, with severe squalls at times. The centre of the cyclone passed northwards to the W. of the Hebrides, where the barometer sank to the almost unprecedentedly low reading of 27·860 inches at 9 p.m. At 10 p.m. of the 16th the barometer stood at 30·400 inches in Dublin—a range of 2·1 inches in five days. On the 24th a remarkably deep depression travelled eastwards up the English

Channel, causing a destructive easterly gale on the Kentish coast. This disturbance was not much felt in Dublin, where, however, it caused heavy rain at noon of the 24th. Temperature was much lower during the second half of the month, but scarcely any frost occurred, owing to the frequent bourrasques and constant S.W. to W. winds. Between the 7th and 10th of the month the interesting astronomical event of the simultaneous appearance in the sky of the four principal planets was observed after sunset. Mars and Saturn formed one group in the S.E., while to S.S.W. Venus and Jupiter, not far from each other, shone brilliantly in the twilight. The crescent moon was seen near Venus and Jupiter on the 9th and 10th. A bright meteor appeared to E. at 8 p.m. of the 24th. Hail fell on the 21st, but no sleet or snow was observed in Dublin. Only on one day, the 24th, was there a fog in this city.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

SOPHORA SPECIOSA.

DR. H. C. WOOD, of Philadelphia (*Phil. Med. Times*), has made a partial chemical study of this plant, from the beans of which he has obtained an organic principle, an exceedingly active poison, the minutest speck producing in two minutes almost entire paralysis in the frog. One-twentieth of a grain of a very impure specimen produced in a half-grown cat deep sleep, lasting many hours. As this substance is not soluble in water but is soluble in acidulated water and is precipitated by alkalies, and as it dissolves freely in ether, imparting to it a decidedly alkaline reaction, it must be looked upon as an alkaloid. Dr. Wood proposes for it the name of *Sophoria*. He obtained it of a greyish-white colour, but did not succeed in crystallising either it or its acetate. Its reactions, as far as he has examined them, are as follows (the tests were made by placing a speck of the alkaloid upon a porcelain plate and applying the reagent):—With concentrated sulphuric acid, no colour. With chromic acid and concentrated sulphuric acid, a dirty, deep purple, passing rapidly into bright green, then into bluish, and finally into yellowish brown. With tincture of the chloride of iron, a deep, almost blood red, after a time acquiring an orange tint. With nitric acid, no colour. With chromic and nitric acid, a very faint, evanescent reddish colour. With nitromuriatic acid, a dirty reddish brown. From the solution of its acetate, compound tincture of iodine throws down a yellowish precipitate. He has made physiological experiments with an alcoholic extract of the bean upon the lower animals sufficient to outline its general action. In

frogs it produces a rapid loss of reflex activity and power of voluntary movement. The loss of power is not due to any action upon the motor nerve-trunks, as after death these were found to preserve their normal susceptibility. Further, tying the sciatic artery upon one or both sides of the frog did not influence the action of the drug upon either voluntary or reflex movements. This would indicate that the poison is a spinal sedative, and has little or no effect upon either motor or sensitive nerves. In all cases the heart continued beating long after the cessation of respiration. Upon mammals the effect varies somewhat in accordance with the dose. An amount of the extract estimated at two grains (?) produced in a full-grown tom-cat, in one minute marked weakness in hind-legs; in two minutes inability to stand, with evident effect upon the respiration; in three minutes convulsive movements, with loss of consciousness, continuing with ever-increasing embarrassment of the breathing for three minutes, when all attempts at respiration ceased. The heart kept on beating for one and a half minutes longer. The pupils were unaffected at first, afterwards dilated. In small quantity the extract produces in the cat vomiting, great muscular weakness, profound quietude and deep sleep, lasting some hours and ending in recovery. In dogs the symptoms were similar to those noted in cats. Death always took place through respiration. In a single cardiac experiment the drug had no decided effect upon the blood pressure until towards death, but appeared to accelerate the cardiac beat.

H. M. J.

ERGOT IN HÆMORRHOIDS.

DR. SANSERY suggests (*Phil. Med. Times*) the use of ergotin in suppositories, four grains in each, to be used, first night and morning, subsequently at night only. He has successfully treated in this manner four cases of hæmorrhoids, and hopes that others will test the matter.

CONSTANT CURRENTS IN CHRONIC LEAD-POISONING.

M. SEMMOLA, of Naples, adopts the following method:—The patient is put in a bath of tepid water (75° Fahr.), acidulated with nitric or sulphuric acid. The bath is put in communication with the negative pole of a Wollaston's pile. The positive pole is applied to the patient's tongue. Every day the process is repeated. Founding his views on the property belonging to constant currents of rendering more active nutritive changes in the organisation, and consequently increasing the rapidity of disassimilation, he believes that he can in this way eliminate lead by the natural secretions, particularly those of the kidneys.—*Ann. de Chim.*

S. W.

THE DUBLIN JOURNAL

OF

MEDICAL SCIENCE.

FEBRUARY 1, 1878.

PART I.

ORIGINAL COMMUNICATIONS.

ART. IV.—*On Alternate Paralyses.*^a By GEORGE SIGERSON,
M.D., Ch.M., F.L.S., M.R.I.A., &c.

THE designation *paralysie alterne*, which Professor Gubler gave to a certain class of diseases, characterises with equal fidelity the relative position of the lesion and the effect produced. It has sometimes been translated “cross-paralysis” by British authors, but this name does not accurately render the original. Whilst “cross-paralysis” simply indicates phenomena of decussation, the adjective “alternate,” borrowed from the science of botany, where it marks a mode of phyllotaxis, not only shows that opposite sides of the body are affected, but also that the central lesion operates at different relative heights. On this account, mirroring to the mind, as it does, the complex conditions of the case, I have preferred it to the more familiar designation, which may prove misleading by its very simplicity.

History.—Wherever a disease is emerging from obscurity into light, it is both interesting and useful, I believe, to acquaint ourselves with at least the general facts of its history, and the labours of those who have assisted in enlarging our knowledge. The manner in which difficulties have been removed by others may help to show us how to get rid of those which yet remain. In the field

^a Read before the Medical Society of the King and Queen's College of Physicians, Wednesday, December 5, 1877. [For the discussion on this paper see page 147.]

on which we have entered the workers have not been many, for the occasions have been rare.

The earliest author of note, so far as I am aware, who has referred to lesions affecting the pons Varolii, was Yelloly, who, in 1808, read before the Medico-Chirurgical Society of London a paper entitled "A Case of Tumour in the Brain, with some Remarks on the Propagation of Nervous Influence."^a His patient having suffered from occasional attacks of pain in the head for a twelvemonth, was surprised one morning on awaking to find his vision double, and his left eye drawn inwards; paralysis and numbness of the right hand, arm, and leg followed, with slight stammering and a small degree of distortion of the mouth. The pupil of the affected eye was in its usual state. After some time the patient was taken with convulsions, became insensible, and died. On dissection the brain was found unusually firm, with half an ounce of water in the ventricles. On the tuberculum annulare a tumour about the size of a hazel-nut was observed sunk into its substance on the left side, at its posterior part. "This tumour extended to the corpus pyramidale of the same side, pressing upon and entirely obscuring the left abductor nerve." From the description it is obvious that the case was one of alternate paralysis. Yelloly, however, in a lengthy discussion of the method of propagation of nervous influence, diverges widely from the right path. Inclining to the views of those anatomists opposed to decussation, he quotes only to reject the experiments of Lorry on the medulla oblongata.^b Whilst recognising, of course, the effect of the tumour on the left nerve of the sixth pair, he volunteers the assertion that he thought it likely, from what he considered the analogy of the other nerves which arise from the medulla oblongata or spinalis, "that under no circumstances of immediate pressure on the origin of one of the nerves of that pair would the opposite eye be affected."

In 1810 Dr. Vieusseux, of Geneva, contributed an account of his own case, under the heading of "History of a Singular Nervous or Paralytic Affection."^c Three years before he had felt severe pain in the gum, just under the third double tooth on the left side of the lower jaw. A month later it returned whilst at

^a Medico-Chirurgical Transactions. Vol. I. Third Edition. P. 183.

^b Lorry. Sur les Mouvements du Cerveau. Mémoires des Savants Étrangers. T. III. 1760.

^c Medico-Chirurgical Transactions. Vol. II., p. 247.

dinner, and was followed by a general sensation of cold; it came on again in the evening, with acute pain in the internal angle of the left eye; disappeared, and returned with general perturbation, giddiness (during which he saw objects reversed), nausea, vomiting, evacuations, loss of voice (though not of articulation), and difficulty of swallowing liquids. Afterwards he suffered from hiccup for a time. The diseased tooth was extracted, and half a spoonful of black blood came away, but the following symptoms persisted till the time of writing, that is, for three years after their invasion. The left half of the face was almost altogether insensible to pricking or scratching. This insensibility prevailed over the left half of the forehead, of the nose, of the upper and under lip, of the chin, and over the left ear. The eye was partly shut, the corner of the mouth slightly drawn downwards, the tongue when put out was turned rather to the left than to the right, but to a very trifling extent. There was a sensation of numbness in the left hand, particularly on the radial side. The left half of the trunk and lower extremity preserved their sensibility, but were weakened, so that the leg dragged in walking. On the right side, on the contrary, the face had perfectly preserved its sensibility, whilst there was hemianæsthesia of the trunk and lower extremity, as if a plane divided the body from nape of the neck and top of the sternum downwards. Pricking and scratching were unfelt; a blister on the epigastrium pained only on the left side of the mesial line. Dr. Vieusseux could perfectly feel the pulse of a patient, and says he had in no degree lost the sense of touch.

The sense of temperature (if I may so speak) was greatly lost. There was thermo-anæsthesia to a high degree, for he could grasp hot objects with his right hand which would burn his left, and he could even keep the former in boiling water until warned to withdraw it by a disagreeable sensation different from that of burning. There was also the opposite condition (for which I have to invent the term *psychro-anæsthesia*), for the power of receiving a sensation of cold was equally lost—thus a cold phial seemed lukewarm in the right hand. On plunging into a very cold bath the water appeared almost warm to the right side.^a In a hot bath it appeared neither hot nor cold to the right.

^a *Psychro-anæsthesia* may be manifested to such an extreme degree that cold bodies shall seem unbearably hot. Thus Abercrombie relates that Dr. Falconer had a patient who, after an attack of paralysis, felt cold objects intensely hot; when he first put on his shoes he felt them very hot, and as they gradually acquired the temperature of his

It may be observed, in conclusion, that, notwithstanding some abnormal sensations in the head, such as giddiness, Dr. Vieusseux retained command of his intellectual faculties; so that, whilst comforting himself with the belief that his disorder was only a peculiar nervous affection, and that the brain was not originally affected, he took every precaution lest a local congestion might supervene.

We have here two cases which may almost serve as types:—

1. Of *alternate motor-paralysis*, which Professor Gubler has described; and

2. Of *alternate sensor-paralysis*, to which I would direct attention.

There is also a third form, to which, indeed, Dr. Vieusseux's case somewhat approaches, and of which I shall, in concluding, give an example, from my own observation. This is a case,

3. Of *alternate mixed paralysis*. In this form diminution of the powers of both movement and sensation is observable to a more or less marked extent.

Proceeding with our historical sketch, we next find a case, reported by Salter, in 1815,^a in which the pons had undergone considerable change from induration; but, as there were other brain-lesions, there is little to be elicited from the complex symptoms. Abercrombie gives three or four instances in which there was found abscess, hæmorrhage, tumour, or induration of the part; but his statement of symptoms does not supply the details we require. Thus, where the eye-muscles are affected, the patient is simply said to "squint"—in what manner, or with what eye, we are not told.^b Bright, in 1831, mentions the case of a child, Mary Jessett, aged six years and a half, whose right leg and arm, though not absolutely paralysed, were embarrassed and stiff, and sometimes stretched out rigidly. The left eye was noticed to remain open when she slept, and the left side of her face seemed more affected, more flaccid, and with less expression than the right. The left

feet they appeared to him to be cool. Conversely, *thermo-anæsthesia* may be equally intense. In a case I saw with my late lamented master, Dr. Duchenne (de Boulogne), everything the patient touched, such as paper, wood, &c., felt like ice. The term *psychro-hyperæsthesia* may be applied to a case related by Abercrombie, where there was such susceptibility to cold impression in the arm that the slightest breath of cold air excited convulsions.

^a The Edinburgh Medical and Surgical Journal. Vol. XI., p. 469. 1815.

^b Abercrombie. Pathological and Practical Researches on Diseases of the Brain and Spinal Cord. 1828.

hand was always cold. On autopsy, the left side of the tuber annulare was found swollen and diseased, and the 4th, 5th, 6th, and 7th nerves were all implicated.^a Cruveilhier, whose attention had been called to the case of a child, in 1822,^b mentions, in his "Anatomie Pathologique" (1835-42), several instances of more or less extensive lesions of the pons. Of these, that of Jean Mourgue, aged eleven, is most free from complications, and furnishes a decipherable illustration of alternate paralysis.^c Cruveilhier's mind, however, had not been fully awakened to this question; his inquiries were chiefly directed to discover whether the protuberantia had any influence over articulation, and, if so, what that influence was. His cases, moreover, were generally complex, or the lesion such as to cause total ruin of the part; to this, probably, is due his candid declaration that he knew no characteristic signs for apoplexy of the protuberance.

The next observation is one of much interest; it was reported, in 1841, by Dr. Annan, in an American medical journal.^d A negress, aged twenty-eight, whilst engaged in washing linen, experienced an acute pain in the right side of her head, fell down, and remained insensible for twenty-four hours. On recovering her senses, she found she had entirely lost the power of moving her left arm, and, in a great degree, that of moving the left leg. The extremities on the right side were not affected, but the right side of the face was paralysed. The tongue, when protruded, inclined much to the right. The right eye was open and became inflamed; she could not hear with the right ear. With respect to sensibility, it is stated that "the sensibility of the left side was destroyed, and likewise that of the right side of the face." This being the case, the motor and sensory parályses were coincident. She lived over a year after being struck down; but when admitted to Baltimore Hospital, two months before her death, her powers of utterance and deglutition, as well as of mastication, were gravely affected. At the autopsy a tumour, seated in the membranes, was found on the right side of the tuber annulare and medulla oblongata, extending from the point of emergence of the fifth pair (which it covered) over the whole of the right side of the tuber beneath, and two-thirds of the medulla oblongata; with the latter it had become incorporated, and the

^a Bright. Reports of Medical Cases. Vol. II., p. 48. 1831.

^b Nouvelle Bibliothèque Médicale. T. IX., p. 314. 1825.

^c Cruveilhier. Anatomie Pathologique. Vol. I., Livr. XVIII., v. q. Complex Case of Françoise Paulin, aged seventy-one. Vol. II., Livr. XXI.

^d The American Journal of the Medical Sciences. P. 105. Philadelphia, 1841.

whole region pressed by it had been softened. The anterior tract was pulpy, and the ramollissement had invaded the posterior tract, but became less as it approached the posterior surface. The roots of the 5th, 7th, 8th, and 9th nerves, on which the tumour pressed, were softened—so also was the surface of the root of the right crus cerebelli. The left side was not perceptibly affected.

This was a clear case of that third form of alternate paralysis, which I have termed *mixed*, and which is subdivisible into two kinds—

(a.) *Coincident alternate paralysis*, in which the sensory and motor affections coincide, as in the present case; and

(b.) *Sensory-motor alternate paralysis*, in which they cross like the strokes of an X.

Dr. Annan appends some lucid remarks to his report. Accepting as a fact the still-contested doctrine of decussation of the corpora pyramidalia (which, though proclaimed by Mistichelli and Pourfour du Petit, in 1709 and 1710, was not finally demonstrated by Lockhart Clarke until 1857), Annan used it to explain the motor symptoms. He adds: "But as we have no facts proving a similar interlacement of the fibres of the posterior or sensory tract, it is not so easy to discover how it happened that the right side was not deprived of sensation. Motion and sensation were unimpaired in the extremities of the side diseased; they were both destroyed in the same parts of the opposite or left side. Are we not justified from this," he adds, "in making the inference that there is a decussation of the filaments of sensation, as well as those of motion?" I quote this, partly to do honour to the author, partly to show that the logic of the physician may sometimes precede the scalpel of the anatomist in the elucidation of obscure points of structure. Lockhart Clarke, however, has since placed the matter beyond the possibility of doubt, when he demonstrated that the anterior pyramids, whilst consisting of some non-decussating anterior fibres, some decussating fibres from the anterior commissure, and a large quantity of lateral fibres, also included decussating fibres from the posterior white and grey columns.

In the following year Longet, with the object of throwing some light on the physiology of the region in question, published, with remarks, a case borrowed from Dr. Carré, of coincident alternate paralysis.^a About the same time, Romberg^b mentions that he had

^a Longet. *Anatomie et Physiologie du Système Nerveux*. Vol. I., p. 448. 1842.

^b Romberg. *A Manual of the Nervous Diseases of Man*. Vol. II., pp. 408, et seq. Sydenham Society, 1853.

a patient, a Pole, who presented similar symptoms. The sensory and motor affections were alternate, coincident, and complete. For the first time we have mention made of a diagnosis being pronounced previous to the autopsy. "The complication of paralysis of the left extremities," he observes, "with a paralytic affection of the right side of the face, allowed me to assume a disease at the right base of the brain, involving the peripheral distribution of the nerves given off there." Proceeding with an anatomo-physiological clue, he was able to indicate, with much accuracy, the seat and extent of the lesion, but erred as regards its nature, supposing it to be a fungous tumour, and rejecting the idea of hæmorrhage—"because," he wrote, "neither the experience of others nor my own afforded an instance of hæmorrhage, at the base of the brain, after which the patient survived ten weeks with so extensive a residuary affection." On examination it was, however, found that the cause of an apparent tumour, seated in the right half of the pons Varolii, was a large coagulum, disposed in layers, as in an old aneurism.^a The credit of having been the first to diagnose the lesion, in a case of alternate paralysis, must, I believe, be accorded to Romberg.

With the exception of some brief reports of cases, in which symptoms and lesions are noted, there is nothing worth remark until we come to Professor Gubler's work. Dr. Mahot, of Nantes, in 1840; Drs. Vaussin and Stuart Cooper, in 1846; M. Bourceret, in 1847; and Professor Forget, of Strasbourg, in 1850—contributed each an account of symptoms and *post mortem* signs observed during this period.

A new epoch in the history of the disease began in 1856, when Professor Gubler, having had his attention awakened to the subject by the examination of some patients, published his first investigations.^b His conclusions were not immediately accepted. Trousseau, himself basing his opinion on Blondeau's case,^c whose symptoms Gubler considered to have been wrongly interpreted (an opinion which I share), contested the correctness of his views. This opposition fortunately caused Professor Gubler to deepen and widen his researches, and, in 1859, having collected all the cases reported in France, he published a second memoir,^d in which he

^a Reference for delineation of parts is made to Casper's *Wochenschrift für die gesammte Heilkunde*. 1842.

^b Gubler. *De l'Hémiplégie Alterne*, etc. Paris: Masson. 1856.

^c *Gazette des Hôpitaux*. 29 Mai, 1858.

^d Gubler. *Mémoire sur les Paralysies Alternes en Général, et particulièrement sur l'Hémiplégie Alterne*, &c. Paris: Masson. 1859.

may be said to have legislated for the disease with considerable, if not complete, success.

A brief analysis of his views may not be considered out of place, more especially as his work is not known in these countries, except through the pages of Trousseau. In British essays I have failed to find reference made to one who gave the disease a name, and whose investigations secured it a place in classic treatises. The title of Professor Gubler's first memoir "*On Alternate Hemiplegia considered as a sign of Lesion of the Protuberantia Annularis, and as proof of the Decussation of the Facial Nerves,*" is sufficient evidence of the purpose of his essay. Romberg's observation was not known to him, any more than to Trousseau, and thus he made a re-discovery; but he did much more, for having collected a number of facts he extended the scope of his investigations, and proceeded to generalise. Having under treatment a patient named Barrois, in whose case there was paralysis of the right extremities and of the left side of the face, he considered that the lesion, situated above the bulbus rachidicus, should also involve the trunk or radicular fibres of the left facial below their point of decussation. Were the lesion above it there would have been right facial paralysis. This being absent, and there being no disturbance of the intellectual faculties, he considered he could exonerate the cerebrum. No symptom indicated that the medulla oblongata was implicated. Hence he believed he could limit the lesion to the point of emergence of the left facial, and the adjoining region, not trespassing beyond the median line. The diagnosis was found to be correct. Exactly at the same time Professor Forget, of Strasbourg, published a similar case, in which an equally accurate diagnosis was made. Having collected six other examples more or less complete, and discussed their lesson in detail, Professor Gubler came to certain conclusions, which I summarise:—

1. Cerebral hemiplegia, properly so-called, is always unilateral.
2. In the rare cases of alternate hemiplegia the protuberance is injured.
3. The lesion is always or usually on the side opposite to the extremities—hence its action is crossed as regards the extremities, but direct as regards the face.
4. As disturbance of facial sensibility and motility may exist as well with isolated alteration of the protuberance as when the nerve-trunks are themselves involved, alternate hemiplegia should therefore be transformed into a sign of lesion of the protuberance.
5. On comparing the decussate or cross-action, in facial paralysis, of lesions

situated in the cerebral hemispheres, with the direct action of lesions of the mesencephalon, it is obvious that the facial nerves decussate in the substance of the isthmus. 6. This pathological induction is justified anatomically by the researches of MM. Vulpian and Philipeaux. 7. Pathology proves that the decussation must be complete.

It would be impossible to admit the second proposition, at all events, as universally true. In justice to the author, however, it should be stated that he anticipated the possibility of exceptions; thus in a note he observed that it might perchance happen that a lesion in each hemisphere would, by cross-action, affect one side of the face and the opposite extremities, and thus produce an alternate paralysis. Admitting it to be possible, he rejected this supposition, however, as contrary to all probability.

Mimetic Alternate Paralysis.—Further research revealed the fact that we may have alternate paralysis without pontine lesion. The cause may be either peripheral, cerebro-peripheral, or perhaps purely cerebral, as suggested above. 1. As an example of the first form I may quote the case of a patient in the Hôpital Beaujon, reported by M. Dulaurier (1858). This individual having dislocated his left shoulder by a fall, found that his arm remained almost completely paralysed after removal of the bandage. During convalescence he exposed himself to chill currents of air, and got paralysis of the right facial nerve. With regard to the second or cerebro-peripheral form, one case may be quoted from Dr. Duchenne (de Boulogne).^a The patient, on whom portion of a house had fallen, had the right facial nerve injured by a sharp splinter of wood, and facial paralysis ensued. He dislocated the left humerus in falling on his shoulder, and paralysis of the left arm and leg supervened, owing, as it was diagnosed, to a lesion of the opposite hemisphere. Another example, the most curious of any, we owe to the scrutiny of Dr. Benson, of Dublin,^b who is entitled to claim priority in the discovery of mimetic alternate paralysis. In his case the autopsy revealed the existence (1) of a small tumour not larger than a hazel-nut, pressing on the antero-superior part of the hemisphere opposite to the paralysed side; (2) of another and lesser tumour attached to the inferior surface of the dura mater, just where it

^a Electrification Localisée. Observation CLXXII.

^b Benson. Dublin Medical Press, April 28, 1852, and Gazette Médicale de Paris, 1852, p. 674. Dr. Benson made as exact a diagnosis as was possible under the circumstances; he mentions that the patient had had syphilis, and that the tumours were all of "a scrofulous nature."

lines the Gasserian ganglion; and (3) of a third tumour in the Fallopian aqueduct compressing the facial nerve. Professor Gubler frankly avowed that, in such a case as this, the illusion would have been almost inevitable, and diagnosis almost impossible. He might, in fact, have gone further, and said that it was quite impossible to distinguish, in such cases, between cerebral and peripheral paralysis at the time he wrote.

I may here point out that a differential diagnosis between alternate paralysis of cerebral or cerebro-peripheral origin, and alternate paralysis of pontine origin, which it was impossible to make when Dr. Gubler wrote, can now be made. The method of research by electro-muscular exploration, which we owe to my late regretted friend and master, Dr. Duchenne (de Boulogne), supplies us here with an invaluable resource. Had it been understood in those days, the subsequent discussion between Trousseau and Gubler would have been greatly simplified—Blondeau's case, for instance (where the former considered there had been left facial paralysis, and the latter right facial contracture) would have been elucidated. Failing this resource, on considering the chapter of exceptions, Professor Gubler felt coerced to narrow his definition, and to qualify it. The term alternate paralyses should be restricted to such as had but a single lesion, which could only exist in the protuberance; such as had multiple lesions, having no anatomical connexion, should be refused the name, and called bifarious or alternate. It seems to me that it would be better, in the latter case, to use the term mimetic.

True Alternate Paralysis.—True alternate paralysis, Professor Gubler points out, takes as its anatomical basis the fact that the decussation of certain nerves is effected higher up than that of others—hence the same lesion being post-decussate as regards the former, and pre-decussate as regards the latter, will have direct action in the first case, and cross-action at a lower level in the last. This being the case, it is plain that whilst the term “alternate paralysis” is descriptive of the disease, the appellation “cross-paralysis” is a misnomer.

Applying the information derived from observation of the position of recorded lesions and their effects, Professor Gubler considered he might divide the protuberance into a superior and an inferior region. The boundary between them would be a transverse line drawn across at about one centimetre from the upper border. The antero-superior region he terms *peduncular*. Here he supposes fibres of the facial to be before decussation, basing his

supposition on the fact that in cases where there was lesion of the peduncle (*crus cerebri*), and of the adjoining portion of the pons, the effect produced was simple decussate unilateral hemiplegia. In the other or postero-inferior region, which he terms *bulbar* (from its contiguity to the *bulbus rachidicus*), and which is much the larger, the facial nerve after decussation is found, and here a single lesion in one lobe will produce both direct and decussate phenomena, or, in other words, alternate paralysis.

Different Species.—In illustration of his views, Professor Gubler was able to collect about a score of cases, several of which, however, are incomplete. He pointed out that we may have different species of the genus, alternate paralysis, depending on difference in the lesion's seat. Thus, there may be paralysis of the left common oculo-motor, with total hemiplegia of the right side, resulting from lesion of the left *crus cerebri*, encroaching or not encroaching on the adjacent portion of the pons. Professor Vulpian has more recently suggested other examples.^a Thus, a lesion of the pons or upper part of the *bulbus* may produce direct paralysis of one of the external oculo-motor, or of the smaller fasciculus of one of the trigeminal nerves, and cross-paralysis of the extremities. So likewise there may be, according to the position of the lesion, direct paralysis of the sensitive portion of one of the trigemini, of one of the *pathetici*, of one of the *hypoglossi*, of one of the acoustic nerves, &c.—in a word, of one of the cranial nerves—simultaneously with paralysis of the opposite extremities. Professor Vulpian refers to these as alternate “hemiplegias.” I have termed them alternate “paralyses,” inasmuch as Professor Gubler restricts the former appellation to cases in which one half of the body is affected—*e. g.*, one side of the face, and one (opposite) side of the body. He also proposed a classification, to which I shall refer again, and which, I think, taking into account the cases since recorded, may be somewhat augmented, if not amended.

In the interval between the publication of the first and second essays of Professor Gubler, appeared Dr. Lockhart Clarke's conclusive researches on the minute anatomy of the medulla oblongata, which put an end to doubt on the question of decussation.^b In the following year, 1858, M. Brown-Séquard's researches on the physiology and pathology of the *protuberantia annularis* were published, in which, recognising the fact that experimentation on

^a Vulpian. *Leçons sur la Physiologie du Système Nerveux*. P. 527. Paris. 1866.

^b *Philosophical Transactions*. London, 1858.

animals could here be of little avail, he carried out an inquest on pathological facts, and drew some memorable conclusions. These he further illustrated and expanded in his lectures on disease of the pons Varolii, delivered in 1863.

Differential Diagnosis.—A most important epoch in the history of the disease is marked by the discovery of Dr. Duchenne (de Boulogne)—a discovery made on the path indicated by Marshall-Hall^a—that electricity may be used not only for the cure, but also for the differential diagnosis of such paralysis. I have already indicated how some misunderstanding might have been avoided had this method been known twenty years ago; even now, however, it cannot be said to be much employed, and, I believe, the case of my patient offers the first example in which it was utilised in alternate paralysis—for Dr. Duchenne (de Boulogne) records no cases examined by him after Gubler had explained the cause of the disorder. In such cases it is of much advantage to know exactly what we have to deal with—whether, for instance, the disease be of cerebral origin or not.

Again, suppose a patient with hemi-facial paralysis comes before us, can it be contended that the diagnosis is always easy? Deviation of the tongue and uvula has been relied upon; but, on examining the case-reports of careful observers, I find it was sometimes present and sometimes absent in cases of alternate paralysis. Then it has been authoritatively, and I may say universally, laid down that paralysis of the orbicularis palpebrarum is conclusive evidence—"a pathognomonic sign," Todd says—that the lesion is not cerebral. Récamier first announced its existence to be a distinctive symptom of lesion of the seventh nerve. Undoubtedly it is evidence that more of its power is arrested than is usually observed with cerebral lesion; but Dr. Duchenne (de Boulogne) justly remarked that it has lost much of its value since Dr. Duplay's work,^b in which hemi-facial paralysis in the aged was shown to be correlated with a cerebral lesion in several instances—yet in all these cases the orbicularis was paralysed. I may add that I have myself had a young patient stricken with complete hemiplegia of the right side who could not close the right eye; the indications here, in spite of this symptom, pointed to a pre-decussate cause, and

^a Marshall-Hall. *On the Condition of the Muscular Irritability in the Paralytic Limbs.* London, 1839-1840.

^b Duplay. *De la Paralyse Faciale produite par une Hémorrhagie Cérébrale chez les Vieillards, in l'Union Médicale.* 28 Août., 1854.

a brief examination of the state of electro-muscular excitability determined the question in the same sense. On the other hand, we may have a partial paralysis of the portio dura, which shall not be the consequence of a cerebral lesion, but which could not well be distinguished except by this method of research; for it sometimes happens that a cerebral lesion affects one side of the face without producing hemiplegia of the body.

Thus, as Dr. Duchenne (de Boulogne) observed, a facial hemiplegia may be symptomatic—firstly, of (*a*) a brain lesion (blood-clot, syphilitic tumour or exudation, tumour, cyst, or tubercle), or of (*b*) a lesion of the superior pontine-peduncular region; and, secondly, of (*c*) the inferior or bulbar-pontine region, or of (*d*) the facial nerve at its point of emergence, or (*e*) of the same nerve in its course. If farado-muscular contractility remain intact in the paralysed facial muscles, circumscribed lesions of the first class must be diagnosed, and extension of the paralysis to the same side of the body—*i.e.*, the side opposite to the lesion—may be apprehended. There is, then, a fine distinction to be made between (*a*) brain lesion, and (*b*) lesion of the peduncular region. In the latter case, owing to the disposition of the third pair in the superior pontine region, paralysis of the oculo-motor on the side opposite to the hemiplegic paralysis—or, in other words, an imperfect alternate paralysis—may be found.

If the farado-muscular contractility be greatly diminished or abolished in the paralysed facial muscles, then lesions of the second class are indicated. To discriminate between these a careful study of the symptoms and history of the case may be needed. Thus rheumatic peripheral paralysis, *a frigore*, may be usually eliminated without difficulty; then paralysis of the sixth, or alternate symptoms, inculcate the pontine-bulbar region. In this way, by a process of exclusion, even injuries to the nerve in its course may be diagnosed with some certainty. Electro-diagnosis enables us to broadly distinguish between what I may term pre-decussate and post-decussate lesions,* a distinction which it is of great importance to make in many cases.

From Opium-poisoning.—There is another distinction, on the

* This distinction is curiously displayed in alternate paralysis, where farado-muscular irritability is greatly decreased or abolished in the paralysed facial muscles, whilst the muscles of the paralysed opposite extremities retain it, more or less completely. This might appear to indicate that some anatomo-physiological alteration takes place about the decussation point which has not been sufficiently elucidated. It is, however, more correct to suppose that, with a pre-decussate lesion, some of the radicular fibres escape injury, and keep up connexion with the nutrient nerve nuclei.

making of which the life or death of a patient, and, perhaps, the liberty of others also, may depend. This happens, for instance, where a person is found comatose, with contracted pupils, and the suggestion of opium-poisoning is made. Thus Dr. Taylor, in his work on poisons, quotes cases where a victim of such poisoning was treated for apoplexy, and where an apoplectic sufferer was considered to be under the influence of poison. Dr. Hughlings Jackson mentions a case where a gentleman, in a strange place, after taking a glass of ale, cried, "I am poisoned!" fell to the ground, and became insensible.^a He had but one eye, and its pupil was contracted. The stomach pump was used. He died in a few hours, and his death probably saved some innocent person from a charge of murder, as a blood-clot was found in the pons Varolii. The matter is made the more urgent, because not only is the proper treatment neglected (as has been remarked), but the treatment proper in one case is very likely to be injurious in the other. Now, in this acknowledged difficulty of diagnosis, I would appeal with confidence to electro-muscular research, which has not yet, so far as I know, been employed. The rapid diminution of the electro-muscular contractility in cases of pontine lesions has been noticed, and this I should not anticipate in opium-poisoning. But, in addition, we have another resource in verifying judgment, and that will be found in the difference between the greater degree of electro-muscular contractility in the muscles of the face than in those of extremities, whereas the influence of opium would be general.^b

With a view to determine the general features of the disease with

^a London Hospital Reports. Vol. I., p. 350., v. q. Medical Times and Gazette. P. 214. 1863.

^b Attention should also be given to the interesting observation of Dr. Liouville (*Gazette des Hôpitaux*, Feb. 23, 1873). A patient having been brought into hospital in a state of insensibility, his urine was withdrawn by the catheter, examined, and found to contain albumen and sugar. Hæmorrhage of the pons Varolii was consequently diagnosed, and correctly, as the autopsy proved. A similar test should not be omitted in doubtful cases, for it may sometimes prove equally useful, as in my friend Dr. Liouville's case. Albumen and sugar have also been found after cerebral lesions, but this only enlarges the scope of the test without detracting from its value in cases of opium or drink-poisoning. It will be borne in mind, however, that both substances have been found in the urine after an epileptic seizure, and that the fact of their absence is not evidence against the existence of a pontine lesion. Thus, in the case of Dr. Desnos (*l'Union Médicale*, Mars 27, 1873), there was hæmorrhage of the pons Varolii, though neither albuminuria nor glycosuria had existed. The position of the lesion must be taken into account, as was proved by the admirable researches of Professor Claude Bernard. The discovery, by Quincke, of one of the above-mentioned products in the urine, after poisoning by morphia, renders it urgent to have a more certain test, such as that suggested in the text, of electro-muscular exploration.

which we are here concerned, I have collected, collated, and compared all the cases recorded; they do not exceed fifty, so far as I can make out. Having analysed them as best I could (the observations are often imperfect), I tabulated the principal symptoms, and shall now state the chief results of this investigation.

Symptoms: their variety.—In the majority of cases there had been *premonitory symptoms* of various kinds, such as headache, giddiness, megrim, facial neuralgia, fixed pain, nausea, or vomiting; but in many cases there seems to have been no warning, or else the warning was overlooked. The *invasion* may happen in different ways: the patient may fall to the ground insensible, or he may remain conscious; again, the disease may manifest itself less strikingly—an eye may be affected with strabismus, half the face may be paralysed, and then, after a few hours or days, the opposite extremities may be stricken. In several cases the upper affection preceded the lower. With respect to the state of the *mental faculties*, it may be laid down as a general rule that the intellect remains undisturbed. In some cases, however, it is enfeebled, and becomes obtuse and slow in its operation. The power of *phonation* is occasionally preserved intact, but usually there is some hesitation, slowness, or embarrassment, which may proceed to stammering, and degenerate into muttering of unintelligible sounds in bad cases. *Deglutition* is sometimes difficult; sometimes, on the contrary, it is easy. *Taste* is said to be good in certain patients and impaired in others; but it is only in a few cases that any effort was made to determine its state on different sides of the tongue. Romberg's patient is recorded to have perceived the savour well on both sides. There may be *deviation of the tongue*, and then it is usually mentioned as deviating away from the facial paralysis, but in one or two cases (as in Annan's) it was turned towards it. Sometimes the tongue is *protruded* slowly. In several cases, however, there was no deviation whatever. The uvula also may deviate or not, but it has been rarely examined. The *sense of smell* is frequently declared to be good—sometimes it is said to be impaired—but scarcely any attempt has been made to compare its condition in one nostril with that in the other. *Diplopia* existing, the *vision* is, of course, interfered with; otherwise it is generally good. In one or two cases only was it troubled. The pupil of the paralysed side of the face is often noted as contracted; but, on the other hand, careful observers have reported it normal in several instances; and, again, it has been noted as contracted on

the opposite side, and occasionally in both eyes. Sufficient attention has not been usually given to discriminate between the *auditory capacity* of the ears; hence there can be no exact value set upon a general statement that the hearing is good, or that it is impaired. Where the distinction was made, I find that sometimes the ear on the paralysed side of the face was affected with more or less deafness, sometimes that on the opposite side. In a few cases the *bladder* was voided involuntarily, in two or three it was paralysed and distended, and in one or two mention is made of involuntary passing of the fæces. The urine has been found at times to show traces of albumen and sugar; in other cases these substances were absent.

No absolute rule.—It results from the foregoing analysis that no absolute rule can be laid down respecting the presence or absence of any of the symptoms mentioned. The intellectual powers, indeed, are generally preserved intact, but they have been in some few instances found enfeebled. Paralysis of the orbicularis is usually present in alternate *hemiplegias*; but as it has been sometimes found correlated with cerebral lesion, it is impossible to accord it that pathognomonic value which Récamier and Todd claimed for it. But will not paralysis of the sixth nerve supply us with a diagnostic sign of absolute value? Dr. Brown-Séquard truly says that this nerve is very frequently affected in lesions of this region, and he has added:—"Paralysis of this nerve never exists except with disease of the pons Varolii."^a There are, however, exceptions to this rule to be found. M. Badin d'Hurtebise has described this paralysis as sometimes essential,^b and Dr. Beyran has proved that it may occur in venereal patients, owing to pressure on the nerve, in its course, from exostosis, periostosis, or gumma.^c

Add to this that we may have paralysis of the sixth nerve indicative of something quite other than pontine alternate paralysis—of locomotor ataxia, for instance. Diplopia and strabismus, as Dr. Duchenne (de Boulogne) observes, may be amongst the earliest symptoms of this disorder, and may stand almost, though not altogether alone.^d To increase the difficulty, paralysis of the third

^a Medical Times and Gazette. Vol. I., p. 247. 1863.

^b Thèse de Paris. 1849.

^c Grisolle. Traité de la Pathologie Interne. Vol. II., p. 805.

^d In the year previous to his decease, Dr. Duchenne (de Boulogne) mentioned to me that he contemplated omitting the epithet, *locomotrice*, as he had, on further investigation, discovered that the disease might exist without the lower extremities being necessarily affected.

and fifth, and even of the seventh and eighth nerves, may be met with in exceptional cases of ataxia. The pontine region is, of course, affected, but that the lesion of the sixth, at all events in the outset, is peculiar, may be inferred from the fact that, in one ataxic case, abducent paralysis and diplopia were cured or disappeared for months, appeared again, and again gave way, up to five times. It should be remembered that Dr. Duchenne (de Boulogne) was finally forced to abandon his early belief that a diagnostic sign would be found in the presence of amblyopia or amaurosis, in ataxic strabismus, by which to distinguish it from simple strabismus—or, I may add, from the strabismus of alternate paralysis.^a

Temperature.—The question of temperature-change is one which requires further investigation. In 1858, Dr. Brown-Séquard, commenting on some cases, observed:—"We will point out as one of the first and surest symptoms of lesion of the protuberantia, in one of its lateral halves, a lowering of the temperature in the opposite side, which is sometimes considerable."^b This, he showed, proved that Schiff was wrong in not admitting that the vaso-motor nerves ascended to this region. He inferred that they decussated before arriving there, on noting the case of Barrois, where the paralysed arm was found warmer than the other.^c On comparing all the reports accessible, I find that in the majority of cases no notice has been taken of temperature change. In one case (abscess) the temperature is mentioned as normal; and in two the paralysed limbs are said to be the warmer. In others they are colder, as in the case which came under my own observation. Further information on the subject of temperature change, arising from pontine lesion, is desirable, more especially since so much knowledge has been acquired from the work done by Professor Charcot and the Salpêtrière school, as regards temperature in cerebral disease,^d and by Mr. Hutchinson, in reference to nerve-lesions.

^a My patient, the Rev. Mr. L——, having consulted a late distinguished oculist in reference to his eye, the latter informed me that he found no abnormal appearance in the fundus oculi, from which he concluded (erroneously) that there was no central lesion.

^b *Journal de la Physiologie.* T. I., p. 525.

^c *Ibid.* T. II., p. 133.

^d Professor Charcot has shown (*Note sur la Température des Parties Centrales, &c.*, 1867, et *Leçons Cliniques*, 1869), that three periods may be observed in connexion with central temperature in the apoplectic state, caused by hæmorrhage or ramollissement. They may be called:—1. Period of initial fall; 2. Period of oscillation; 3. Period of elevation. In the first hours following the attack, the central temperature falls below

Nature and Position of Lesion.—With respect to the kind of lesion reported present, as the immediate cause of the disease, “tumours” take the first rank, being the most numerous; close upon them follow hæmorrhagic foci; after these come tubercles, ramollissement, and then (but rarely) abscess. There is no mention made of sclerosis, which is, indeed, a matter of very recent research. Judging by the description given, however, I believe we may class some, at least, of the so-called tumours as nodules of sclerosis.

It seemed interesting (as cerebral hemiplegia is most common on the left side) to inquire whether, in cases of alternate paralysis, one oblique half of the body was more frequently affected than the other. Curiously enough, the balance was found to be nearly even. There was a slight predominance of what (borrowing a term from heraldry) one might call bend-dexter paralysis—that is, there were a few more cases in which the right face and left extremities

37·5° C.; in the second period, lasting some days, it oscillates between 37·5° and 38° C.; in the third it rises rapidly to 39°, 40°, or 41° in fatal cases. Dr. Bourneville, a distinguished pupil, continuing these researches (*De la Température, &c.*, 1872), notes that in cases of ramollissement the initial fall is not so great, whilst in the second period there may be a sudden elevation to 39° or even 40°, with evening or morning remissions to the extent of 1° C., and a slow return to normal, with irregular oscillations; in the third period the temperature increases, but more slowly than in hæmorrhages.

An exception to the rule as regards temperature in hæmorrhage seems to me to have a certain significance. In one case (Observation IX., *ibid.*), in which the principal lesion was a hæmorrhagic focus, occupying nearly the whole thickness of the pons Varolii, the initial fall was observed (36·4° C.), and an oscillation or stationary period; but the third period offered an altogether extraordinary irregularity. After having slowly risen to 38·1° the temperature gradually declined to 37·2°, or the normal standard, where it stood at death. In discussing the cause of this exception, my friend, Dr. Bourneville, rejects the supposition of a pressure-paralysis of the bulbus, and inclines to consider the phenomenon as due to the probable extension of the pontine hæmorrhage into the middle crus cerebelli. In another case (Observation VIII.) there had been extension into the inferior crus, and a focus in the superior crus, without noticeable effect on temperature of the limbs, or deviation from the customary ascent, in the third period.

There may, in fact, be another interpretation. Judging from the case noted by M. Brown-Séquard, from others, and from my own, I venture to suggest that there is a tract of the pons Varolii, injury to which has the effect of causing a decrease of temperature in correlated parts of the body. This view is supported by the results of one of the experimental lesions made by M. Claude Bernard, on the floor of the fourth ventricle, whilst studying the production of glycosuria (*Leçons*, 1858). In one case, where the lesion was exceptionally placed, there followed coma, and an exceptional temperature-fall (p. 428). That the artificial lesions might occasionally reach the region with which we are chiefly concerned, is manifest from another case (p. 411), in which internal strabismus and facialis paralysis were produced. Whilst maintaining this opinion, it is not of course denied that a diminution of temperature may also be produced, more or less immediately, by certain brain-lesions.

were affected. The majority is so minute that the question of equality is scarcely affected.

Causes.—Sex, age, and, perhaps, race, may be enumerated amongst the predisposing causes. By far the larger number of the patients were males; most were well advanced in years. Tubercle, however, has brought a few children into the category, and syphilis, by inducing disease of the arteries, some young men.^a Heart disease (hypertrophy) is accused in two cases.^b Sudden frights and movements—hurrying to avoid danger—have induced pontine paralysis in a few instances. If we may infer anything from the records as regards the preference of this disorder for one race over another, then we should say that the Celtic constitution is more readily affected than the Teutonic. It is quite possible that the records do not supply sufficient data to support anything more substantial than a probable conjecture. On the other hand, we know that Paris is acknowledged to be the great centre of nervous disease, and that the influence of race seems to be admitted with respect to the preference of paralysis agitans for the (so-called) Anglo-Saxon race.

Prognosis.—The questions of treatment and recovery have not been much dwelt on in reports of cases—apparently because the great majority, whose cases were thought deserving of notice, died. Some, however, have been known to survive very serious lesions for many months, and some have passed out of sight, apparently improved. Although the disease is one of great gravity, it cannot be considered in all cases hopeless—it might even be expected to become almost hopeful if *preventive treatment* were early undertaken. With that view it would be needful to watch for premonitory symptoms, the existence of which, I am convinced, has been often overlooked.

Classification.—That there should be much diversity of symptoms is what ought to be expected, when we recollect that the protuberantia is a microcosm in which a small change in the size or position of the lesion must produce varied results. It may be compared to a junction, where the shifting of an obstacle will interrupt lines running in divergent and widely different directions. However, by a comparison of consequences, singular facilities are given us for tracing up to the exact position of the hindrance.

^a J. J. Browne. *Journal of Mental Science*. 1875.

^b Meynert, in H.H. Virchow und Hirsch., *Jahresbericht*, p. 65, 1874; and Dr. Bourneville, *De la Température dans l'Hémorrhagie*, &c., p. 91, 1872.

The first who endeavoured to reduce the seeming chaos of symptoms to order was, undoubtedly, Professor Gubler, who, in 1859, sorted out and systematically arranged the several varieties of alternate paralysis, resulting from lesions of the bulbar region of the mesencephalon, known to him. The following is his classification:—

ALTERNATE HEMIPLEGIAS—LESIONS OF THE BULBAR PORTION OF THE PROTUBERANCE.

Simple—Unilateral Lesions.

Motor paralysis of extremities. *Lesion of anterior columns.*

Sensory paralysis of extremities. *Lesion of posterior columns.*

Sensory-motor paralysis of extremities. *Lesion of sensory and motor fasciculi.*

Motor paralysis of face. *Origin of trigeminus intact.*

Sensory-motor paralysis of face. *Lesion approaching the crus cerebelli.*

Double—Bilateral Lesions.

Imperfect: one side of face intact, the opposite extremities weakened. *Lesion chiefly unilateral, but exceeding the median line.*

Perfect: two alternate hemiplegias, one complete, the other beginning, crossed like an X. *Lesion occupying the breadth of one lobe, and a large portion of the other; or a less portion of one, and nearly the entire height of the other.* (The latter form, when in a more advanced state, and with a more extensive lesion, constitutes a generalised paralysis, simulating general paralysis.)

According to our present knowledge, the enumeration is incomplete in this scheme. For instance, sensory paralysis of the face is not mentioned, and Dr. Vieusseux's case would be excluded—so, also, would be the case I am about to record.

For purposes of descriptive reference, where a short title is desirable, I venture to suggest the following classification:—

SIMPLE ALTERNATE PARALYSIS (MOTOR OR SENSORY).

Bend-dexter: right face and left extremities.

Bend-sinister: left face and right extremities.

COINCIDENT ALTERNATE PARALYSIS.

Sensory and motor paralysis of same regions.

DOUBLE ALTERNATE PARALYSES.

Complete.

X-shaped paralysis: both sides of face and extremities of both sides of body affected.

Incomplete.

V-shaped paralysis: both sides of the face affected.

Y-shaped paralysis: both sides of face and the extremities of one side of body affected.

λ (*lambda*)-*shaped paralysis:* one side of face and extremities of both sides of body more or less affected.

By prefixing the terms “motor,” “sensory,” or “sensory-motor,” where required, we shall thus obtain short descriptive names, which may be found useful.

CASE OF SENSORY-MOTOR Y-SHAPED PARALYSIS.

The Rev. Mr. L—, aged sixty-five, rural clergyman, is a man of athletic frame, sanguine temperament, and active habits. No hereditary tendency traceable. Used claret in moderation. With respect to premonitory symptoms, he had suffered from nothing, except occasionally from colic, which, however, could scarcely give reason for suspecting gastralgic crises. Another exception must also be made, and one probably of more consequence. For some seven or eight years he had been subject to periodic paroxysms of asthma, coming on fortnightly, which were relieved by vomiting. There was heavy perspiration; cold and wetting induced a fit. Advised to smoke, he found his stomach tolerated neither tobacco nor stramonium. This seemed, in my judgment, to indicate some irritability of the pneumogastric nerve, arising probably from a central disturbing cause, such as the formation of miliary aneurisms or growth of a tumour. The possible correlation of a pontine lesion with the production of asthmatic paroxysms is shown in the circumstances of a case reported by Dr. Desnos, in 1869; the patient died in asthmatic paroxysms, and, on autopsy, a blood-clot was found in the upper half of the pons Varolii.^a In the shoulder of the extremity, afterwards affected, the Rev. Mr. L— had occasionally suffered from so-called “rheumatic” pains.

The invasion came on thus:—Having gone, in good health and spirits, on a hot July day in 1876, to a fair, and bustled about there, he became greatly heated, and started to walk quickly homeward, expecting a car to overtake him. He became fatigued, sat by the way-side for an hour and a half waiting for a car, and then drove home in the chill of the evening, without his usual cloak. He got a trembling, felt cold, and when he arrived was “quite stupid.” A hot foot-bath was supplied. However, he remained in this stupid state for three or four hours; then he became unconscious, and so continued for four or five hours. He recognised the doctor when he came. An enema having been administered, he remained in bed for four or five days suffering from no pain. When he got up he perceived he had a tendency to fall forward, and was somewhat confused in mind. On the return of consciousness he had become aware that his face was, as he expressed it, “distorted,” that the faculty of vision was disturbed, as he saw objects doubled. He also suffered from some unpleasant sensations in his right arm, and was troubled by incontinence of urine—this fluid being passed every eight or ten minutes, a glassful at a time.

When I first saw him, in the last week of August (the attack had happened in the first week of July), the latter trouble was greatly

^a Desnos. *L'Union Médicale*. Mars 27, 1873.

diminished; he was firm upon his legs, and his general health seemed good. He came under treatment, in Dublin, in November.

Paralysis of the left side of the face seemed complete; this was due to the abolition of power in the seventh nerve. [In order to be strictly accurate, and to prevent ambiguity, it might be well to adopt the German custom in such cases, and say *facialis* (rather than *facial*) paralysis.] The left half of the brow was smooth, and had consequently a more youthful appearance than the right. The left eyebrow was immobile, and hung somewhat lower than the other. The eye itself, affected by internal strabismus, was staring open, as he could neither depress the upper, nor raise, nor tighten the under eyelid; the latter therefore sank loosely outwards and downwards, the resulting deformity being greatest towards the inner angle, which was enlarged. There was but little, if any, overflow of tears. The eyeball was quite sensitive to touch, and readily became blood-shot; hence it was necessary to keep the lids closed and covered, which precaution likewise prevented the disagreeable effects of diplopia. Latterly, however, he had not been annoyed by this, probably because he had unconsciously succeeded, by altering the position of his head, in adapting the movements of his right eye to the maimed movements of his left—just as in the effort to close the eye he rolled the pupil up under the upper lid, which he could not depress.

Both nostrils were open, even on inspiration; the right, however, was the larger, and was somewhat drawn upward. When the face was in repose, the ridge of the nose was deviated to the paralysed side. The left angle of the mouth, whence the saliva dribbled, was depressed. The cheek, somewhat sunken above, was apparently swollen in the anterior half, and formed a roll or ridge along the under jaw. The region of the masseter preserved its tone and firmness. Beneath the left maxilla there was a slight degree of flaccid fulness.^a

The patient could not open his mouth much more than was sufficient to permit the protrusion of the tongue, which was weak, and slightly deviated to the left side. He could not turn up the tip. There was little, if any, deviation of the uvula; the left arch, however, seemed larger and not so concave as the right.

Sensitiveness to touch and to pain was greater on the left (paralysed) side of the face, than on the right.

^a Compare with this paralytic laxness the effects of the spasms of the *portio dura*, described by Graves, where twitching of the facial muscles occurred, and "it was also observed that the *platysma myoides* participated in each spasmodic contraction, and its fibres were seen throwing themselves out strongly in relief, in well-marked bundles." The patient "also complained that the *os myoides* was sometimes pulled towards the right ear." (Clinical Lectures, p. 431, Dublin, 1864). The condition of this submaxillary region has not hitherto been remarked in cases of facial paralysis, yet a knowledge of it ought to be serviceable in diagnosing the extent and position of the lesion of the facial nerve.

The true condition of the right side of the face readily escaped observation, and had not previously been remarked. The patient at first merely complained of some deafness in the right ear. He casually mentioned that shaving was a rather anxious operation, inasmuch as he was liable to cut his right cheek. It seemed strange that the flaccid left cheek should give him no trouble, whilst the apparently sound cheek was a cause of anxiety. On examination, I found that this was due to the existence of profound anæsthesia and analgesia, which, extending over the region of the inferior maxilla (the shaving region), ascended the temple, involved the external ear, and invaded a limited space behind and above. A certain degree of pallor characterised the region of the lower jaw and ear. The patient complained that the parts felt thick, stiff, and cold, and that he was annoyed by a sensation of "dead numbness" in the ear as well as by deafness. The frontal, orbital, and upper maxillary regions were spared. The right anterior two-thirds of the tongue were distinctly less sensitive to touch and taste than the left. The upper lip retained its sensation, nor was the lower lip noticeably insensitive.

The right anterior cervical region preserved also a certain amount of sensibility, but anæsthesia and analgesia were well marked in the right lateral and posterior portion, over the right scapular and pectoral half of the thorax, and generally throughout the right half of the body, though but scarcely noticeable in the lower extremity.

In the right upper extremity the diminution of tactual and algic sensibility was very distinct, but the presence of thermo-anæsthesia was a yet more remarkable phenomenon. The patient became aware of this loss of heat-sense, if I may so call it, in the following manner. While breakfasting, he took the eggs served to him in his right hand, and found they were cold. Naturally annoyed, he sent them away, but when others were presented in their stead, they seemed equally cold. This led to a lively discussion, until he happened to pass one to his left hand, when he found it too hot to hold. This fact will give us a means of estimating the extent to which this special form of sensibility had diminished.

The most troublesome disturbance of sensation from which he suffered was a tingling or formication in the arm, of varying intensity and unstable position. It was tolerably constant in the neighbourhood of the elbow-joint; it sometimes ran down the radial side of the forearm, and there was painful hyperæsthesia in the tips of the inner fingers. Thus, in August, he avoided shaking hands with the right; nor could he hold an object between the thumb and forefinger because of the hyperalgesia of the index-tip.

Trophic disturbance supervened. The skin at the root of the nail became red, shining, and inflamed, then a superficial whitlow formed on the index. It healed readily enough, but, in November, similar hyper-

algæsia was felt in the medius-tip, which resulted, as before, in the formation of a superficial whitlow in this finger.

The various movements of hand and arm could be performed, and there was no marked wasting—still the dynamic power, as tested by Charrière's dynamomètre, had fallen to less than one-half of the amount which might be regarded as normal in a man of his age, stature, and strength. Thus, the first effort gave 30 kilogrammes, or about 70 lbs. The dynamometrical test obtains an important supplement when we also consider, as Dr. Duchenne (de Boulogne) wisely suggested, the amount of nervous incitation available. This is a factor which, as far as I can see, is usually overlooked. In the present case, nervous incitation was rapidly exhausted; on a second effort, the dynamic loss was 10 lbs.; and on a third, it was 15 lb. more—thus making 25 lb. loss.

The general functions of the body seemed fairly performed. The pulse was normal. There was no more tendency to constipation than what might probably be accounted for by restricted exercise during the stormy weather. To the disturbance of innervation, however, appears attributable a liability to copious perspiration, which he fostered by a predilection for hot fires and heavy bed-clothes. He complained that in the mornings the right arm was covered by a cold clammy sweat, but the lowness of its temperature would probably suffice to account for the peculiar unpleasantness.* The right upper extremity was considerably colder than the left.

Diagnosis.—The complexity of symptoms present in this case offers, in its interlacement of clues, the means of checking and verifying a judgment, so as to enable us to obtain a result almost as convincing as a necroscopical examination. In the first place, with respect to the facial nerve, its lesions may be enumerated as superficial, petrosal, post-decussate, and pre-decussate. The first can be eliminated by a consideration of the history of the case, and collation with the other symptoms; the second, by a similar method, and by the result of treatment; the last, by collation with the accompanying symptoms and the state of electro-muscular contractility. Several objections lie against pressure on the nerve after its emergence by an intra-cranial tumour—hence, by a process of exclusion, we come to a post-decussate lesion within the protuberance, which opinion is corroborated by other symptoms, such as the abducent and partial-trigeminal paralysis.

It might, perhaps, be argued that the dimidiated diminution of sensibility gives reason to refer back to a cerebral cause, and to suspect a hæmor-

* In M. Senac's case (pontine hæmorrhage), the skin is described as cold, and covered with an abundant perspiration. In one of Professor Gubler's cases there followed a "sudoral state," after prickling sensations, in the paralysed members. In Dr. Bourneville's case (Observation IX., pontine hæmorrhage), the arms were cold, the left remarkably more so than the right, and the skin was as if "viscous."

rhagic focus, involving Charcot's region, the lenticulo-optic third of the internal capsule. Professor Charcot has, however, pointed out that in cerebral hemianæsthesia the optic and olfactory nerves are involved,^a whereas in the present case there was no such consequential injury to sight or smell.^b Again, we had not here a complete, but an incomplete, hemianæsthesia. The indications furnished by the paralysis of the trigeminus are, however, of themselves almost sufficient to denote the lesion. The question might arise: "Since the paralysis is confined to the inferior maxillary nerve, may it not happen that pressure by an abnormal growth on that nerve, as it passes the foramen ovale, furnishes a peripheral cause?" That supposition is eliminated by the fact that, whilst the motor and sentient nerves are here united, the paralysis affects only the sensitive filaments of the inferior maxillary nerve; consequently, we must refer back for the cause to the posterior roots of the fifth. Again, inasmuch as the ophthalmic and superior maxillary branches are spared, it follows that the lesion is not at the point of emergence, where the whole root would most probably be involved, but must be sought deeper, where it may affect only one of its strands in passing through the protuberantia, or bear upon one of its nuclei.

Position of Lesion.—These reasons are given in some detail. It has been acknowledged that the symptomatic group, which represents outwardly the affection, may be produced by other lesions, and it is therefore necessary to see whether we cannot arrive at certainty in so grave a question. On the grounds stated, therefore, and taking into consideration the history of the case, I concluded that, after the existence of some miliary aneurisms, there had been hæmorrhage, and that a clot of small size and irregular in shape had formed in the substance of the bulbar region of the pons Varolii, on the left side. It oppressed the post-decussate trunks of the sixth and seventh nerves, encroached on some nuclei cells of a radicular fascicle of the posterior root of the fifth, and, lying more backwards than to the front, chiefly affected the sensory tract. Its presence had given rise to some degree of inflammation, causing referred peripheral sensations in the right shoulder and arm, even in the midst of anæsthesia. The anterior portion of the pons was but little affected.

With respect to the complaint of deafness in the right ear, the auditory nerve can scarcely be inculpated. The deafness, or rather hardness of hearing, gave way so rapidly when the anæsthesia was being dispelled, that it may probably be attributed to paralysis of the nerve-

^a *Leçons sur les Localisations dans les Maladies du Cerveau, faites à la Faculté de Médecine de Paris. 1875. Recueillies et Publiées, par Bournville.*

^b The patient fancied that there existed some decrease of the olfactory power; but, on carefully testing the matter, I found he could perceive a very faint odour equally well in both nostrils.

filaments, which, derived from the otic ganglion, and, indirectly, from the inferior maxillary nerve, proceed to the tympanum.

The lesion was of sufficient magnitude to affect the root of the hypoglossus. The existence of incontinence of urine with the persistence of motility in the lower limb indicates a well-limited lesion. Here I may refer to the theory of Budge,^a who, looking upon the constrictor urethræ and bulbo-cavernosus as the true sphincter of the bladder, traces the nerves that control the contractibility of that organ past the restiform bodies to the crura cerebri. In their course they might be affected by such a lesion as that described.

Nature of Lesion.—The mesencephalon has been found injured by lesions of different kinds: by abscess (rare), ramollissement, sclerous nodule, tumour, tubercle, and hæmorrhage. M. Duret in his remarkable researches on the angiology of the brain, points out that the vessels of the protuberantia, by their separation at right angles, their parallel arrangement, and lack of anastomoses, recall the disposition of the arteries in the brain proper. In the bulbus rachidicus the arrangement approaches more to what characterises the spinal cord. Professor Charcot has shown that, as a consequence of this arrangement, hæmorrhage by vascular rupture, which depends on miliary aneurismal alteration, and ramollissement consequent on arterial blocking, are almost unknown in the spinal cord, occasional in the bulbus rachidicus, and common in the protuberantia. I am led to indicate a hæmorrhagic lesion from certain clinical characters. The previous irritability of this region, revealed by the antecedent disorders, might be equally attributed, perhaps, to the growth of a tumour or the development of miliary aneurisms. The complete relief from his asthmatic paroxysms, which the patient experienced afterwards, seems to me compatible with vascular rupture rather than with other causes.

Treatment.—The patient had been blistered in front of the left ear, directed to keep the eye covered, and been taking phosphorus. This did not, however, dispel the paralytic symptoms, which I found as described. When I saw him in August, for four days, it seemed proper to postpone continuous electro-muscular treatment till a later period—hence I contented myself with applying strictly localised faradisation to the arm and hand. In this way the painful sensations were removed from the latter, and the patient became able to grasp objects and shake hands with comfort; muscular energy was strengthened, and the dynamometrical standard rose steadily from 79 to 95, 104, 115 lbs. When I saw him in November he was still free from the pain in the index, but the dynamic standard had somewhat fallen, being 102—a decrease not surprising; it rose to 110, 119, and 126, during treatment, although no

^a Zeitschrift für Ration. Heilkunde XXI., pp. 3, 134. Pflüger's Archiv. für Physiologie. Bd. II., p. 511.

special application was made to the extremity for that purpose. The nervous incitation had markedly improved; his second efforts now generally surpassed his first. Care was taken to regulate the bowels—a somewhat difficult task, inasmuch as he repudiated aperients which did not operate half-a-dozen times—the surreptitious practice of which doctrine weakened him considerably. Iodide of potassium in draught, nitrate of silver and extract of belladonna in pill, were administered internally, and occasionally the phosphorus pill. Counter-irritation was set up in the nape of the neck. To the parts affected by akinesia and anæsthesia, including the ear, localised faradisation was applied in the manner, and with all the precautions, habitually adopted by Dr. Duchenne (de Boulogne). There was gradual and steady progress, broken only for a fortnight by an attack of tonsillitis (in the paralysed side), which resulted from exposure, while fasting, on a chilly morning. Previous to that, the reflex act of winking had been regained, and next the voluntary power of closing the eyelids was partially obtained—that is, the patient could perform that act in the mornings. The state of the orbicularis palpebrarum and of Horner's muscle was considerably improved. During his fortnight's illness from tonsillitis he fell back not a little; the involuntary power of nictitation, however, continued. This acquisition is of peculiar value when the condition of the eyeball is considered, for the lid is not only its natural protector, not only distributes moisture, but prevents evaporation, and gives periodical repose to the retina.

In the first week of December the patient no longer complained of tingling or formication in the arm, whose dynamometrical standard remained at about 125 lbs., beyond which it seems needless to augment it. Farado-muscular contractility began to show itself, and quickly increased in the buccinator, zygomatic, and supraciliary muscles, which were, in sequence, after the orbicularis. This may seem contrary to Dr. Duchenne's experience, but it is simply due to the fact that more attention was given to the orbicularis, on account of the state of the eye. The state of the external rectus had also evidently improved, inasmuch as the convergent strabismus had decreased. At this period, although the patient had not yet regained voluntary control over the muscles animated by the portio dura, their tone had manifestly improved. The face had, as an observer remarked, "straightened;" the flabby swelling of the cheek and the ridge at the lower jaw had disappeared. Food no longer lodged between the buccinator and the teeth. On the right side of the face the anæsthesia had been more rapidly dispelled; and he could hear the ticking of a watch, at equal distances, with both ears. At this stage, as Christmas was at hand, the patient went home, with the intention of returning in spring—city life being very irksome to a man of his active habits. Some weeks later I received a letter from him,

stating that (contrary to my injunctions) he had been much exposed to the winter cold and wet, and caught cold in his left eye, which had become painful. This appears to have given way to the remedies advised, as he no longer complained of it in a letter received a month or so later, but he mentioned then that he suffered from incontinence of urine, but to what extent he did not say. He still survives.

In conclusion, I may remark that there are several topics of physiological and pathological interest suggested by the incidents of the foregoing case. I shall mention only a few.

1. The term facial or hemi-facial paralysis is ambiguous. It seems to imply that one side of the face is completely paralysed, whereas it is known that the muscles supplied by the trigeminal may be spared. Where facial paralysis is meant to designate paralysis of the facial nerve, I submit that, to avoid ambiguity, the expression *facialis* paralysis may be employed. In the present case the muscles supplied by the trigeminus were not only spared, but functionally exalted.

2. The liability of the left eye to hyperæmia of the conjunctiva furnishes a pathological pendant to the physiological experiments of Magendie, Snellen, and Schiff. It has been proved that section of the trigeminus is not necessarily followed, as at first alleged, by hyperæmia and ulceration of the eye, if only the precaution be taken of keeping it covered. In the present case, of two eyes equally sensitive, one became injured because of the inability to close the eyelids.

3. The closure of the paralysed nostril on inspiration (as mentioned in manuals) is dependent on its form; if normally wide and funnel-shaped, the entrance of a current of air does not necessarily tend to close it.

4. The sense of taste appears to depend, to some extent at all events, on the lingual branch of the trigeminus, inasmuch as its paralysis was accompanied by a diminution of taste, as well as of tactual sense, in the anterior two-thirds of the tongue.

5. The difficulty which the patient complained of in opening wide his mouth appears due to two causes:

- a. Abnormal exaltation of the trigeminus counts for something; this exaltation is similar to what I have found elsewhere side by side with paralysis. It seems to constitute what M. Vulpian, in reference to another subject, terms a "*balancement*." Here it was betrayed by hyperæsthesia of the sensory filaments, and by a quicker response of the muscles to the faradic stimulus than is usual.

b. Akinesia of the muscles, which serve to open the mouth by depressing the lower maxilla, must also be taken into consideration. It is to be expected when the second branch of the seventh is affected. The accuracy of this opinion is borne out by the fact that when the muscles in question were specially treated and their strength augmented, the difficulty experienced in opening the mouth diminished.

6. Part of the deformity noticeable in patients affected by facial palsy is due to the weight of the lax muscles and integument, which tends to drag down the lower eyelid and to deviate the ridge of the nose downwards and towards the paralysed side. Part of it is also due to the surviving action of the quadratus menti and depressor anguli oris, which, being animated by the mental branch of the trigeminal nerve, and no longer equipoised by the tonic force of their antagonistic muscles, depress the angle of the mouth, and give an expression of sadness to the face.

Hence, not only is that distortion, which is observable when the patient speaks, due in part to the action of healthy muscles, as has been remarked, but the action of other sound muscles, together with the weight of lax tissue, helps to produce the deformity which is observable when the face is in a state of repose.

ART. V.—*Facial Paralysis; with Remarks on the Action of Induced Currents and of Interrupted Voltaic Currents.*^a By WALTER G. SMITH, M.D., Assistant Physician to the Adelaide Hospital.

OF the various forms of peripheral paralysis that affecting the facial nerve occupies a foremost place in importance and interest. It offers the most common example of a morbid process engaging a cerebral nerve; its pathology has been carefully studied; it has given birth to an extensive literature of its own; and, more than any other single nerve, has contributed to the elucidation of some disputed questions in neurology. Moreover, from the superficial position of the muscles and terminal motor filaments involved, it lends itself with facility to electrical exploration; and although, even when appropriate treatment is adopted early, a speedy recovery

^a Read before the Medical Society of the King and Queen's College of Physicians, Wednesday, 2nd January, 1878. [For the discussion on this paper see page 150.]

or ultimate cure cannot always be secured, yet on the whole its prognosis is certainly favourable, and in a large majority of cases the results of judicious treatment are highly satisfactory.

The remarkable and extremely characteristic clinical features of facial paralysis have been so often and so well described, that in the following cases I shall restrict myself to a brief summary of those points alone which bear on prognosis and treatment.

CASE I.—Michael K., a sailor, aged nineteen, was referred to me by Dr. Duncan, in January, 1875. A month previously, while at sea, and after some days' exposure to severe weather, he found, one morning, on awaking, that his mouth was stiff. He was conscious of muscular twitchings on the *right* side, attended with acute earache of the same side, but hearing was not disturbed. The face was swelled, and, on observing it in a mirror, the features were drawn to the *left* side. For a week he perceived a remarkably bitter taste in the right side of the mouth.* When he came under observation the paralysis was not complete, and the food did not lodge in the cheek.

Relations to Electricity.—The affected muscles responded readily to a mild faradic current, and much more easily to an interrupted galvanic current than the healthy muscles—*i.e.*, galvano-muscular irritability was increased, and yet faradic excitability was not diminished. Thus, two Leclanché cells excited the sphincter oris on the right side, and were incompetent to do so on the left. In two days after electric treatment was commenced he could move the angle of the mouth, and in less than three weeks he could whistle and wrinkle the forehead, although unable to close the eye until after electrification. At that period he discontinued his attendance.

CASE II.—Mr. T. G., aged twenty-four, sent to me by Dr. T. K. Hamilton, February 18th, 1876. On Saturday night, February 13th, he fell asleep with the right side of his face turned towards the window, which was left partly open. Next morning, on awaking, he felt a dull pain behind the right ear, and, on looking into the glass, he noticed that the face was distorted, and that the right eye was staring. The paralysis spread to all the facial muscles, and there was no impairment of taste or hearing, nor asymmetry of the palate.

Electrical Relations.—Faradic irritability very slightly diminished. With three Leclanché cells slight impairment of galvanic excitability

* Morbid gustatory sensations are only occasionally met with, and it is not easy to account for Niemeyer's statement that "the impairment of the sense of taste, the distortion of the uvula, and deviation of the tongue, though less obvious symptoms, are equally constant." (Vol. II., p. 333.)

was determined, but four to six cells caused distinct contractions on either side of the lips. In other words, irritability towards *both* forms of current was perceptibly lessened. Immediately after the first application he could close the eye better; improvement was rapid, and within ten days scarcely a trace of deformity was visible.

CASE III.—Eliza M., aged twenty, came under observation December 1st, 1875, with paralysis of all the muscles of the right side of the face, of a few days' duration.

Electrical Relations.—Faradic excitability slightly but unmistakably impaired. Current from twelve cells acted well on the healthy side, but evoked scarcely perceptible contractions on the right side. This marked diminution of galvanic excitability within the first week is particularly noteworthy. Immediate improvement attended electrical treatment, and in about a fortnight she was almost perfectly recovered. Two years previously this patient had had a similar attack of facial palsy—also cured by electricity. This is the only case I have met with of the occurrence of facial palsy twice in the same individual.

CASE IV.—Richard P. presented himself at the Dispensary, June 12th, 1876, with incomplete left facial paralysis of about three weeks' standing. At first his hearing was abnormally acute, and the noise of a saw appeared loud and distressing.

Electrical Relations.—Irritability not sensibly diminished either to faradic or galvanic stimulus, notwithstanding the duration of the paralysis. After three days' treatment he was almost perfectly cured, and no distortion was visible on speaking.

CASE V.—John W., a middle-aged man, was admitted under Dr. Head's care, February 8th, 1876, on account of a herpetic eruption, which had come out the day preceding, on the left side of the neck. The vesicular outbreak extended from the clavicle to the jaw, behind the ear, and through the hair, but nowhere did it transgress the middle line. No pain preceded or accompanied the eruption, which ran the usual course of herpes zoster, and faded in about ten days. As the rash subsided, he noticed that his mouth was dragged to the *right* side, and that he could not close his left eye. The paralysis could scarcely be ascribed to the action of cold, for he slept in a bed removed from any window, and usually lay on the left side—*i.e.*, the side of the paralysis. The paralysis in this case was apparently brought about in a reflex manner by the cervico-occipital herpes; and we know that herpes zoster in other parts of the body is occasionally signalled by consecutive peripheral paralyses.

Electrical Relations.—Little or no impairment of faradic excitability was detected; the galvanic irritability was not tested. Speedy improvement set in, and after three weeks scarcely a trace of the affection remained.

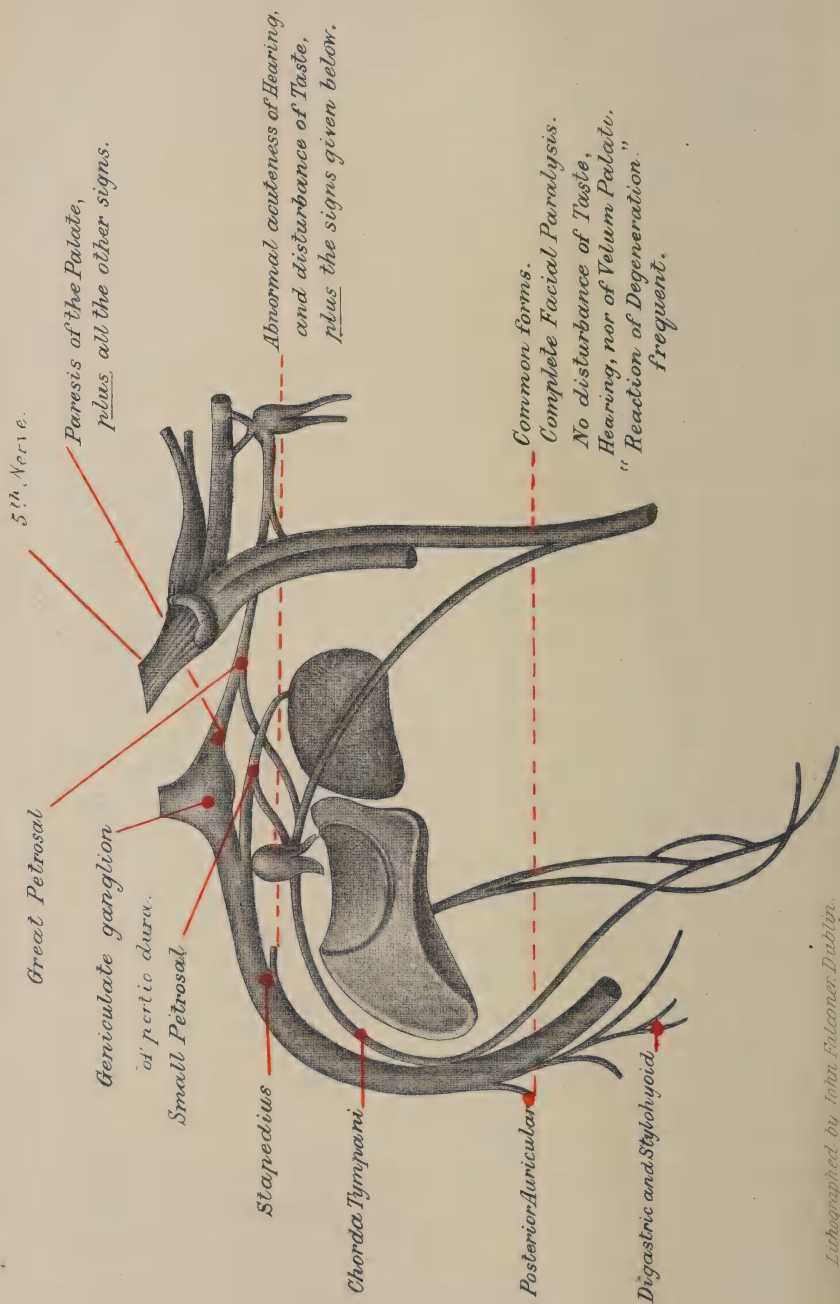
The succeeding two cases illustrate the more chronic forms of Bell's palsy:—

CASE VI.—Mrs. P., aged forty, from Sligo, a delicate, strumous woman, was struck with left facial paralysis on 14th August, 1876, without any definite assignable cause. Having occasion to call aloud, she found that she could not pronounce the words distinctly, and her mouth was twisted to the right side. Attendant on the paralysis was a sensation of dryness in the mouth and on the lips of the paralysed side. She was blistered, dosed with tonics, and magneto-electricity was applied for a time in the country. When I saw her for the first time, on 25th October, 1876—*i.e.*, two and-a-half months after the commencement of the paralysis—scarcely any degree of voluntary power had returned.

Electrical Relations.—Some of the muscles reacted to an induced current, whereas others—*e.g.*, the sphincter oculi and the frontalis—did not. Two to four cells caused galvanic contractions on the paralysed side. Involuntary muscular quiverings were noticed on the left side. In the course of the next three weeks voltaic excitability was considerably lessened, and faint response was elicited by a mild induction current. Faradic irritability gradually increased; motility returned very slowly; and, on leaving town in December, she could close the eye completely; but, even after many months was passed, perfect motility was not restored, as I learned in September, 1877, from Dr. J. MacMunn, to whom I am indebted for the case.

CASE VII.—On the 7th May, 1877, Miss E. C., aged ten, was brought to me, suffering from left facial paralysis, which had developed itself six weeks previously, without any known cause. In the afternoon, while in the house, the child noticed that there was a “catch in the left side of the mouth;” and her sister next remarked “how queer her face was,” and that, when she laughed, the “face was all to one side.” In the course of two or three days the left cheek became tender, and there was pain behind the left ear. This disappeared soon after the application of a leech and blisters. The paralysis increased in degree for a day, and then began to improve up to a certain point. When I saw her she was unable to wrinkle the forehead, to close the eye, or to deviate the lips to the left side.

Electrical Relations.—Faradic irritability considerably impaired; galvanic excitability increased; three cells excited contraction in



the left half of the sphincter oris, but not on the right side. Within a few days some improvement was noticeable, especially in the sphincter oculi, and the left side became so sensitive that two cells provoked pain. Galvanisation was regularly practised almost every day; and on 14th June it was ascertained that, although the current from two to four cells, when *slowly interrupted*, excited the muscles; yet, when *rapidly interrupted* by a suitably constructed wheel, no contractions were produced. It was fully three months after the commencement of the paralysis before she could close the eye voluntarily. Treatment was suspended for a time during the summer, and recommenced in the autumn; and by the end of December (nine months) motility was almost perfectly restored, the frontalis muscle proving the most refractory. Still faradic irritability was enfeebled, and galvanic excitability also was now much lessened—thus exhibiting the curious phenomenon of restoration of motility with persistent loss of electric excitability. To sum up, irritability towards the induced current was throughout much lowered; voltaic irritability was exalted for weeks, and then gradually decreased; improvement was slow but progressive, and an immediate gain was always perceptible, lasting for hours, after each electrical application.

From the above series of cases it appears that the prognosis in facial paralysis varies within wide limits, both as regards duration and completeness of recovery, and that without the aid of electrical investigation no accurate forecast can be made. Why some cases are transient and others persistent, depends partly on the nature, position, and intensity of the lesion, and partly on the relative extent of degenerative changes in the muscle and nerve. The more the structural changes are confined to the muscle, the more rapid will be the recovery. Each case was treated with that form of current to which the muscles responded most readily.

Any attempt to localise the exact seat of the paralysing lesion rests on a precise knowledge of the anatomical course of the portio dura, and of the physiological functions of the nerves connected with it, for the facial nerve is by no means a simple motor nerve.

The diagram (Plate I.) illustrates the principal rules for localisation proposed by Erb, who has added so much to our knowledge of this affection. It is to be noted that Erb refers all the rheumatic cases of facial palsy to slight inflammatory swelling of the sheath of the trunk of the portio dura, which leads to compression and consequent disturbance in nutrition of the nerve; and he tacitly

ignores the possibility of mischief beginning in the peripheral terminations.

Before any opinion of real value can be given as to the probable course of a case of facial palsy, it is simply indispensable to institute careful electrical examinations of the motor function, and, indeed, as Erb remarks, the diagnostic value of electrical investigation cannot be over-estimated.

When we come to study the relative action of direct galvanic currents, and of induction currents, upon a sufficient number of recent cases of Bell's paralysis, we find that the great majority distribute themselves among two classes.

1. Those cases in which muscular irritability is slightly or not at all impaired towards voltaic or induced currents—*e.g.*, Cases II. and IV. These cases recover speedily, and a favourable prognosis may confidently be made, because the absence of electrical disturbance argues the absence of important structural changes in the tissues.

2. Those cases—perhaps the more numerous—in which muscular irritability towards faradic currents is usually markedly impaired, perhaps even appears totally gone, but in which the galvanic irritability is not only preserved, but actually heightened for a time—*e.g.*, Cases VI. and VII.

In the *nerve* the excitability to both faradic and galvanic currents usually disappears, and it is a curious and instructive experiment to test, with suitable electrodes, the relative irritability of the *portio dura* behind the angle of the jaw, and of the muscles on the side of the face.

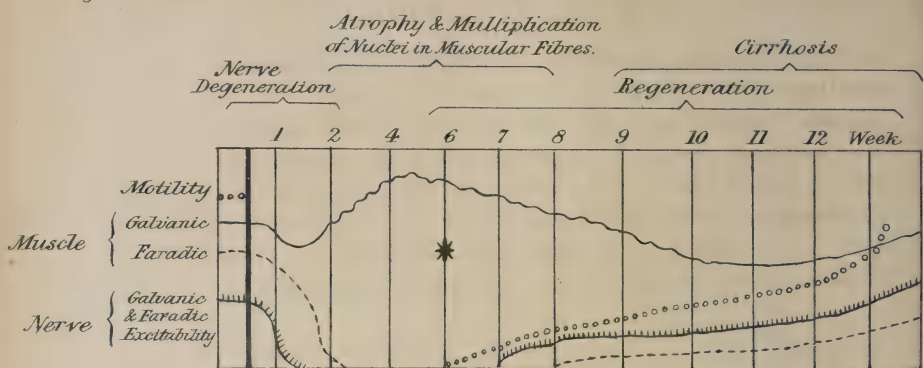
Whenever this curious phenomenon is well marked it is an almost certain prognostic of a tedious course of many weeks or months; but Case I. shows that the rule is not absolute, and that increased galvanic excitability may co-exist with little or no alteration in faradic excitability. May not this be due to a slight primary alteration in the muscular fibres, without any attendant nervous lesion?

It is not to be overlooked that this remarkable alteration in electrical behaviour is not confined to cases of peripheral palsy, and I have seen it well marked in a case of Cruveilhier's atrophy.

First noticed by Hallé, in Paris, towards the close of the last century, attention was not drawn to this remarkable alteration in reaction until 1859, by the publication of Baierlacher's celebrated case. Since that date it has been carefully investigated, clinically

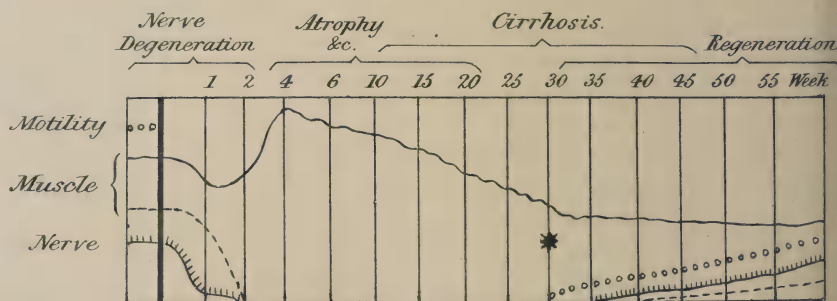
MILD CASE

(Fig. 1)



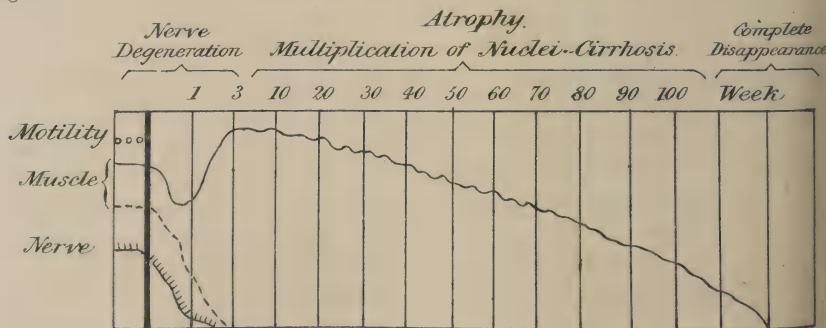
(Fig. 2.)

RECOVERY SLOW



(Fig. 3.)

NO RECOVERY



The * indicates the period of return of motility.

and experimentally, in relation to various forms of paralysis, and is now frequently known by the infelicitous term, "degeneration-reaction,"* given to it by Erb, who has further established that these singular electrical variations are the outward and visible signs of certain histological changes in the nerves (Waller) and in the muscles, in which latter they were first observed by Reid, in 1841. A general idea of these alterations can be gathered from this simple diagram, taken from Onimus; or, more accurately, from



these charts (Plate II.), borrowed from Erb, which represent in a very intelligible fashion the broad relations which exist between motility, electric excitability, and structural changes, in a mild case of facial paralysis (Fig. 1); in a tedious case (Fig. 2); and in an incurable case (Fig. 3). No explanation of this remarkable fact—viz., the muscle failing to respond to the stimulus of a faradic current, while it readily reacts to an interrupted galvanic current, was forthcoming, until 1864, when Neumann accounted for it on a simple physical principle. Neumann proved that if a galvanic current which, when slowly interrupted, excited muscular contraction, was very rapidly interrupted by some mechanical arrangement, so as to approximate induction currents in rapidity, it entirely failed to elicit muscular contraction. He stated also that single shocks of an inductorium failed to cause contraction. The conclusion thence arrived at was as follows:—Induced and galvanic currents exercise different actions upon paralysed muscles, because the muscles in certain diseased states have lost their excitability for even intense currents, *of momentary duration*, while they have preserved it for currents whose duration is longer. It is strange that Niemeyer, in his Text-book (2nd Amer. edit., 1870, vol. II., p. 328), appears to have been unaware of Neumann's experiments; for he observes, that we have no satisfactory explanation of the remarkable fact "that, in many cases of peripheral palsy, the muscles can be made to contract by means of the constant current, while the induced current will fail of effect; and, still more strange, that streams so

* I would suggest the term *para-excitability* in preference, making use of a prefix often employed to indicate abnormality—*e. g.*, par-æsthesia.

feeble as to excite no contraction upon the unaffected side, will induce it upon the paralysed side."

Now there can be no doubt that Neumann's explanation, which has been accepted on all sides, goes a long way, and is in the main true. But it does not include all the facts, and, in especial, it leaves untouched the paradox of paralysed muscles responding to a galvanic stimulus too feeble to affect the homologous healthy muscles—*e.g.*, Cases VI. and VII.

Neither does Erb, in his recent monograph (Ziemssen's Cyclop. XI.), attempt to explain this latter curious pathological puzzle. He merely notes the coincidence between increased galvanic excitability and certain histological muscular changes.

In considering this question we may remind ourselves that physiologically an appreciable duration of current is known to be requisite to produce a stimulating effect; in the case of nerves not less than 0.0015" (König), and, further, that the duration of induced currents is calculated as 0.0042" for the opening current, and 0.0114" for the closing shock. The dynamic changes, therefore, determined in nerve or muscle by the opening induction shock, must be very rapid.

Moreover, since changes are less easily brought about in muscle than in nerve, and the irritability of muscles is less than that of nerves, it follows that length of duration of the stimulus is more necessary for the production of stimulation in the former than in the latter. Thus, Bernstein estimates the length of the wave of excitation produced in a nerve by an electric shock at 15 mm., and the duration of its oscillation from 0.0005 to 0.0006 of a second. For a muscular element he estimates the duration of excitation at $\frac{1}{300}$ of a second, and its length at 10 mm. (Cyon. *Principes d'Électrothérapie*, p. 120). It is not difficult, then, to conceive how pathological alterations in muscular tissue may render the particles of muscle—to speak hypothetically—less mobile than usual to a rapidly intermitting current—*i.e.*, to a series of quickly recurring excitations; or, in other words, the degenerated muscles, their normal properties being qualitatively and quantitatively changed, are exhausted by a rapid succession of stimuli, and, accordingly, lose their capacity to respond. Possibly, in some cases, the muscle is thrown into a state of feeble tetanus, which is mistaken for immobility.

Now it appears to me that in attempting to account for the curious difference in action between a slowly and a rapidly inter-

rupted electric current, stress should be laid not only on the total duration of the current, but also on the *interval* between the successive shocks.

It is not sufficiently remembered that the oscillations of the trembler of an induction apparatus are extraordinarily rapid. They vary from about 100 to several hundred in a second, consequently, within the space of one minute, many thousand waves of stimulation will act upon a nerve or muscle, producing a corresponding number of dynamic vibrations.

Paralysed muscles are not only sluggish to respond—*i.e.*, the period of latent excitation is prolonged, but they are also readily exhausted; and therefore require a sufficient period of rest after each act of stimulation. It has been ascertained that a voltaic current or other stimulus must be repeated at least twenty times in a second to induce tetanus in healthy muscles, corresponding to the number of vibrations of the susurrus denoting tonic muscular contraction; while, in paralysed muscles, five, six, or eight interruptions in a second will suffice (Onimus). We know that muscular contraction is excited only at the opening and closing of a moderate voltaic current, not sensibly during its maintenance; this being so, must not the total duration of the current be of secondary consequence? And again, is not the difference between a slowly and a rapidly interrupted voltaic current chiefly a matter of difference in length of interval rather than in duration?

If variations in rapidity of the oscillations of the current be a principal determining factor of the difference in pathological action of induced and voltaic currents, we ought to be able to demonstrate in a series of cases—

1. That rapidly intermitting induction currents are without effect. This is universally admitted.

2. That rapidly intermitting voltaic currents are without effect. This has been repeatedly verified. Careful observation will, I believe, show that the *first* shock produces an initial contraction.

3. That slowly intermitting voltaic currents produce muscular contraction. This is confirmed by all observers.

4. That slowly intermitting induction currents may produce muscular contraction.

Although Neumann and Onimus have failed to recognise this, I think that I have observed it in a case of facial paralysis, and the point requires reconsideration. An important observation of Brücke deserves mention here. In a frog poisoned by curare, the

muscles will react to slowly broken galvanic currents, and not to ordinary induction currents; but Brücke shows that induced currents will be effective if only they are made to last long enough—*e.g.*, by sliding the secondary coil of Du Bois Reymond's apparatus backwards and forwards over the primary coil arranged as a closed circuit. An induced current is thus produced, not very intense, but lasting as long as the movement of the secondary coil, and *to this induced current the muscles will react.*

Considerable light is thrown upon the question of difference of action of electric currents on paralysed muscles by adopting the view held by Schiff and Brücke that induced currents—*i.e.*, currents of high tension and short duration—are incapable of exciting idiomuscular contraction, and only operate indirectly through the intramuscular motor nerves. But, since interrupted voltaic currents act directly and powerfully on muscular irritability, we can readily understand how that in all cases where nervous lesions precede those in the muscles—and this is typically the case in facial palsy—induced currents lose their effect, and voltaic currents preserve their action. When, on the contrary, the muscular fibres are gravely affected as well as the nerves, irritability disappears both towards direct and indirect stimuli. Moreover, according as the nervous lesion affects primarily the intra-muscular terminations or the trunk of the motor nerve, loss of faradic excitability will supervene sooner or later. [*Cf.* a careful exposition of the whole subject by Onimus (Robin's *Journ. de l'Anat.*, 1874), to which I am largely indebted].

The difficulty still remains of accounting for the extremely curious and interesting phenomenon of increased readiness of response of paralysed muscles towards a slowly interrupted voltaic current. The key to the solution of this problem, which has attracted physiologists and physicians alike, appears to be that offered by Legros and Onimus—*viz.*, that in certain stages of structural and chemical alteration striped muscular tissue comes to resemble in properties the more simply constituted smooth muscular fibre, and therefore is more susceptible than healthy striated muscle to the voltaic stimulus, for, as these physiologists have shown, smooth muscular fibre is more readily excited by voltaic than by induced currents. But the fact that the muscles do respond to interrupted voltaic currents is a sufficient proof that the structural changes in the muscle have not advanced to a profound degree. The well-known circumstance that increased excitability

of the muscles to mechanical irritation may coexist with loss of faradic irritability, points in the same direction, and we may conclude, with Onimus, that to obtain abolition of farado-muscular irritability and augmentation of galvano-muscular irritability, these two causes must concur:—1. Lesion of the intra-muscular nervous filaments. 2. Absence of serious lesion of the muscular fibre (Robin's *Journ. de l'Anatomie*, 1869 and 1874). Certainly the muscular contractions evoked in such cases are less rapid in their origin, more prolonged, less energetic, readily pass into a tonic contraction, and evidently approach in character the movements of unstriated muscle (Onimus, Erb, Vulpian), the properties of which bear some resemblance to those of striated muscle in the embryonic condition. In this way we may form an intelligible idea of the *rationale* of the apparent anomaly, in explanation of which no other hypothesis has as yet been offered.

Finally, there is, I think, ground for holding that facial paralysis may arise in a threefold way:—

1. From affections implicating the nucleus of the facial nerve—*e.g.*, in acute ascending paralysis, tabes dorsalis, and bulbar paralysis.

2. From lesions of the trunk of the portio dura nerve, through cold, injuries, &c.

3. From lesion of the peripheral nerve-terminations, through the action of cold.

GALVANISATION OF THE CERVICAL SYMPATHETIC IN EXOPHTHALMIC GOITRE.

DR. D'ANCONA relates the case of a woman, aged nineteen, suffering for two years from exophthalmic goitre, the usual triad of symptoms being well marked. In spite of all kinds of treatment she had arrived at such a stage of cachexia that her life was despaired of. At length galvanisation with ten elements of Stöhrer's portable battery was tried; and on finding that it was followed by rapid signs of amelioration, it was persevered in for five months. During this time one hundred seances, lasting from three to five minutes each, were given to the patient. She gained thirty pounds in weight; her face lost its paleness, and regained its natural colour; the exophthalmia disappeared almost completely, as well as the enlargement of the thyroid body, and the pulse fell from 130 to 90. Menstruation was restored, and in every respect the health of the patient was entirely re-established.—*Gaz. Med. Italiana pro. Veneto*.

S. W.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Gunshot Injuries: their History, Characteristic Features, Complications, and General Treatment; with Statistics concerning them as they are met with in Warfare. By SURGEON-GENERAL T. LONGMORE, C.B., F.R.C.S.; Honorary Surgeon to Her Majesty; Professor of Military Surgery in the Army Medical School, &c. London: Longmans, Green, and Co. 1877. 8vo, pp. 686.

THE rarity in Ireland of gunshot injuries, particularly of those caused by rifle or cannon, tends, we fear, to make the practising surgeon in this country indifferent to the merits of a work such as Professor Longmore has recently issued. For this reason we are specially called on to notice his book, as, in our opinion, its merits entitle it to a place in even the smallest medical library. We confess ourselves but poor judges of the parts of the work relating to military organisation, but on others we feel justified in expressing our opinion. The chapters devoted to the details of the projectiles used in modern warfare are most interesting, but they are all of those from which a civil surgeon may feel inclined to turn, as outside his proper line of study, unfamiliar as he is in this country with the weapons, their shot and shell, and their wounds. Yet Professor Longmore has the knack of writing so as to lighten the burden of any passage with such accounts of actual practice, so well and shortly put, that one cannot fail to read on with pleasure. The size of the book is such as to render it handy—no small matter when one looks at the colossal volumes of the American War Series—yet in its small compass we find that the essence of these and many other bulky volumes is fairly extracted. To keep the book within reasonable limits as to size, the author tells us that “individual injuries incidental to special regions of the body and their particular treatment are not discussed”—an omission which at first sight is likely to start an objection to the book as one of reference—but in dealing with the principles of general treatment, the author satisfies every inquiry likely to be made of him, unless such as those which fall entirely outside the scope of the work—for

instance, the investigation of minute pathological detail of particular fractures of bone, or of wounds of special viscera; for this kind of information nothing can equal the volumes of the American War Series. Our author, too, meets us on this score, for he says: "I have in my possession the necessary materials for the purpose, which I hope that at some future time I may be able to supply in a separate treatise."

We will quote enough to prove that even to us, insular surgeons, the book can be of use, and interesting:—

"Small shot produce injuries which differ very materially in their appearances and their nature, according to the distance from which the shot have been discharged, their size and number, the charge of powder, the kind of cartridge employed, and the quality of the firearm. . . . When the firearm is loaded, as it used always to be in former days, by pouring the shot on the wad which keeps down the charge of powder, the shot are more scattered than when they are fired from machine-made cartridges, such as are in common use with breech-loaders. One effect of these cartridges is to keep the shot more together, and to cause them to maintain this mutual proximity to longer distances."

On this passage we find the following note, the importance of which cannot but be evident when the statements are placed beside those in the last edition of Taylor's "Medical Jurisprudence" (1873), where this subject is discussed in its bearing on medical evidence:—

"I made a series of experiments with a breech-loading fowling-piece, using one of Ely's cartridges with No. 16 shot, about 250 in a charge, and, comparing them with the well-known experiments recorded by Dr. Lachèse, found the range at which the effects described by him were produced in all instances considerably extended. Thus, Dr. Lachèse records that at a distance of three feet there was no longer any central opening from the shot, while in my trial at fifteen feet there was still a central hole $1\frac{1}{4}'' \times \frac{3}{4}''$ in size, with scalloped edges, and an irregular outline. About 100 shot had passed through this opening; outside the central hole there were about 140 shot openings, some having passed singly; in other instances, 2 side by side; in others, 3 or more together; in others, the shot combining to form an irregular rent, and all these being within a circle of about 7 inches in diameter. At 50 yards about 150 shot out of the 250 pierced a wooden target 8 feet high by 6 feet wide, generally penetrating the wood to their own depth. They were very widely scattered over the target from top to bottom. The experiments of Dr. Lachèse showed great scattering of the shot at very short distances compared with the distances just mentioned."

Taylor adopts the conclusions of Dr. Lachèse in the following passage:—"Dr. Lachèse found, by many experiments on dead bodies, that in order to produce with small shot a round opening, somewhat resembling that produced by a bullet, the discharge should take place point blank at the distance of not more than ten or twelve inches from the surface of the body. When the distance was from twelve to eighteen inches, the opening made was irregular, and the borders were much lacerated; at thirty-six inches a central opening was entirely lost, and the surface of the body was covered by the scattered shot. The effect after this was found to depend on the distance, the goodness of the gun, and the strength of the charge." On this point we need hardly note the exceptional evidence quoted by Taylor of such central round opening as above described being made by a new gun at sixty paces on a sheet of paper; about as much reliance can be placed on it as on the statement of Du Chaillu in his account of African travel, where he says:—"As we were lazily sailing along, I espied two eagles sitting on high trees about eighty yards off. Willing to give my fellows a taste of my quality, I called their attention to the birds, and then brought both down with my double-barrel." When such extremes of evidence as Taylor furnishes are the guides which rule the examination of medical witnesses, it is of much importance that the opinions of Professor Longmore directly bearing on this subject—bearing, too, with all the weight of his recognised authority—should be familiar to our readers.

We extract the following passages from the chapters dealing with the all-important subject of hæmorrhage, as they inculcate most clearly the avoidance of an indiscriminate use of styptics, too often practised in the present day—a practice evidently widespread, too, for we find the reports of the American War Series constantly reverting to and condemning it.

Writing of primary hæmorrhage, the author says:—

"If the bleeding is proceeding from only small arterial branches, some pads of lint or tow, and moderate pressure by bandage, will, in most cases, arrest it; if it be venous, it will be probably found that there is an impediment to the return of the blood through some of the adjoining veins, owing to pressure of tightly fitting parts of the man's uniform, when the release of these obstructions, with the ordinary pad and support, will be sufficient to arrest it. If, however, the hæmorrhage is occurring from injury to a large vessel, it must receive more deliberate treatment. The surgeon should not trust in such a case to styptics and plugging,

which will probably only irritate the wound, cause diffuse infiltration, and conceal what is going on in it; nor even, whenever it can be avoided, to the pressure of a tourniquet; but he should invariably, if possible, secure the wounded vessel at once by ligature. . . . So many accidents have occurred from the causes just referred to, that every field surgeon should hold it to be a sacred duty to secure by ligature, whenever practicable, all wounded vessels of importance that can be got at, before the patients are sent away from his own direct observation."

Again, in the section on secondary hæmorrhage, we find the following:—

"It can only be in rare and exceptional instances that the employment of styptics can be of permanent avail in such cases of secondary hæmorrhage, and, therefore, in all cases where the character of the hæmorrhage points to a vessel of considerable size as being the source of the flow, their employment had better be avoided. Their action on the tissues is to deaden to a great extent their vitality, and they thus tend to place the parts in a condition favourable for the extension of the morbid action which has originated the existing mischief."

We have quoted sufficient to show the practical character of Professor Longmore's book. In closing this notice we will only express our hope that before long we may see the present supplemented by his promised volume on special injuries.

The Australian Practitioner: a Quarterly Journal of Medical, Surgical, and Sanitary Science, for the Australian Colonies.
No. I. October, 1877. Sydney, Melbourne, Adelaide, and
Brisbane: George Robertson. 8vo. Pp. 72.

Cead Mille Failthe! In these words would we welcome the first number of *The Australian Practitioner*, a journal of great promise, and intended to be a medium for the intercommunication of ideas on medical, surgical, and sanitary sciences in the Australian colonies. The interest we feel in this new publication is not lessened when we perceive that it is edited by a distinguished graduate of the Dublin School of Medicine, Dr Samuel T. Knaggs, who seems, so far, to have performed his editorial duties most satisfactorily.

The *raison d'être* of *The Australian Practitioner* is sufficiently expressed in the following sentences from the advertisement of the first issue:—

“Scattered throughout the Colonies are a considerable number of able men engaged in practical and scientific observation of disease—its cause, progress, and termination, as well as the best adapted for its prevention, arrest, or cure. In this Southern climate many diseases, familiar to observers in the old country, reappear in an altered form, and under a variety of guises; often requiring the treatment to be modified to suit climatic influence, diverse mode of life, and variation of social position—furnishing to the general practitioner an instructive study of surpassing interest. Much valuable information gained of such cases by practitioners of long standing in these Colonies is lost through the absence of a Journal where it could be placed on record and saved from oblivion.

“The total neglect, not to say ignorance, of sanitary subjects noticeable throughout Australia amongst the otherwise educated portion of the community is estimated to annually involve a large sacrifice of human life. It is hoped, by the publication of a series of papers on this special subject, and by affording a medium for the accumulation of statistics of deaths by preventable diseases, to greatly strengthen the position of the individual members of the medical profession who desire to use their influence towards a much-desired sanitary reform.”

Under the heading “Department of Public Health,” we find a preliminary communication concerning a report on the public health in New South Wales, which it is proposed to publish in a subsequent number of the *Practitioner*. We may mention that a suggested list of “Preventable Diseases” has been adopted from the “Manual of Public Health for Ireland,” published in 1875. The present number contains several original communications, which will repay perusal; although many will scarcely endorse the diagnosis in a case of extraordinary displacement of the heart, complicated with acute phthisis.

Elements of Agricultural Chemistry and Geology. By the late PROFESSOR J. F. W. JOHNSTON, F.R.S., &c., of Durham; and CHARLES A. CAMERON, M.D., F.R.C.S.I., &c. Tenth Edition. Edinburgh and London: William Blackwood & Sons. 8vo. Pp. 497.

ALTHOUGH this is perhaps in no sense a work on medicine, yet we gladly notice the appearance of its tenth edition, as well on account of the intrinsic interest of the subject-matter as because it is a book which will be of great use to all students of State Medicine.

Chapter VII., on the Composition of Soils; Chapter XI., on Physical Properties of Soils; Chapter XIII., on the Improvement of Soils; Chapter XVIII., on the Improvement of the Land by Irrigation; Chapter XIX., on the Composition of Drainage Water; Chapter XXXIV., on Human Eggesta and Town Sewage; and Chapters XLIII. to XLV., inclusive, on Food and Dietaries, will all be read with advantage by candidates for qualifications in State Medicine and by Medical Officers of Health.

So far as we have been able to form an opinion, we believe with Dr. Cameron—who has brought his special knowledge of preventive medicine and of chemistry to bear on the re-writing of Professor Johnston's work—that “a fair outline of the state of our knowledge of scientific agriculture at the close of the year 1876 will be found in this little book.”

An Atlas of Human Anatomy. By RICKMAN J. GODLEE, M.S., F.R.C.S. Parts I., II. London: J. & A. Churchill. 1877.

THIS atlas consists of illustrations of “most of the ordinary dissections and many not usually practised by the student.” Each Part contains four plates, each plate consisting of two drawings representing the different stages of a dissection. Part I. begins with the posterior triangles of the neck, to which two plates are devoted. The last two plates of this Part illustrate the anterior triangles of each side viewed together. Part II. demonstrates the brachial plexus, the back of the neck, the side of the face; and in Plate VIII., fig. 1, represents the relations and branches of the external carotid, and fig. 2 shows the internal carotid and the distribution and course of the 5th cranial nerve. The plates themselves are very carefully executed, and though none of the drawings exceed half the normal size, the details are clear, accurate, and minute. They are accompanied by an explanatory text, which not only describes the drawings, but also gives a good description of the method which has been adopted to expose the parts represented. This portion of the work might, we think, have been somewhat enlarged with advantage, for the means by which the results have been attained are not always patent to the student. The part of the text which treats more directly of the general anatomy of the parts exposed, contains much interesting matter, is well written, and will repay a careful perusal. The work is specially intended as a guide to *students* in their own dissections, and it will,

no doubt, prove most useful to them. We fear, however, that this professed object will, in a great measure, be defeated by the price which the publishers have thought fit to attach to it. Being designed for *students*, it should be brought as much as possible within their reach, and we fear that but few, comparatively, will be inclined to pay 7s. 6d. for four anatomical plates, when this has to be repeated twelve or thirteen times before the series is complete.

The Student's Manual of Venereal Diseases. By BERKELEY HILL, and by ARTHUR COOPER. London: Smith, Elder, & Co. 1877. 8vo. Pp. 97.

THIS little work purports to be "a concise description of those (venereal) affections, and of their treatment," and we have no hesitation in saying that the authors have been thoroughly successful in their undertaking. It is necessary to observe that they employ the term "Syphilis" only when speaking of the general disease, while they use the term "Chancre" exclusively as denoting the local contagious venereal ulcer. A handy Formulary at the end enhances the value of this capital epitome.

St. Thomas's Hospital Reports. New Series. Vol. VII. Pp. 392. London: J. & A. Churchill.

THIS volume contains, besides the summaries of medical and surgical cases, thirteen papers of a very miscellaneous nature—from a discussion on the true sound of the Welsh "Ll" to an account of "Kakke," a disease lately observed in Japan. We are glad to observe that Mr. Fras. Mason is following up the excellent paper on "Harelip," which he published in last year's Reports, by another, equally good, on "Cleft-palate," in the present number. Mr. Wagstaffe also continues his observations on "Anatomical Variations." A paper on the "Presystolic Bruit," by Dr. Charlewood Turner, will be found well worth consideration by medical retrospects. On "Intra-cranial Aneurisms" Dr. Peacock displays a profound acquaintance with the statistics of a very difficult subject.

PART III.

HALF-YEARLY REPORTS.

REPORT ON MEDICAL JURISPRUDENCE.

By STEWART WOODHOUSE, M.A., M.D.; Lecturer on Institutes of Medicine, Carmichael School of Medicine.

POST MORTEM IMBIBITION OF POISONS.

DR. REESE, Professor of Medical Jurisprudence in the University of Pennsylvania, has published a series of experiments originating in a medico-legal question of a peculiar kind. A case of alleged poisoning by arsenic occurred in one of the Western States of America, in which the suspicions were exceedingly strong that the poison had been designedly introduced into the body after death for sinister purposes. The deceased was an aged man who had been treated in his last illness for phthisis, his physician subsequently testifying to his having died of this disease, and to his having presented none of the symptoms of arsenical poisoning before his death. The body had been buried some three or four years, during all of which long interval of time no suspicion of foul play appears to have been entertained. In the meanwhile the widow married again, after which, for reasons dependent on questions of inheritance of the property belonging to the deceased, the suspicion of poisoning was bruited about, the woman was accused of the crime, and the body was exhumed for judicial examination. The autopsy revealed a remarkable state of preservation of the body—a circumstance of itself well calculated to sustain the suspicion of arsenical poisoning—and as if to remove all shadow of a doubt, this poison was actually detected in the stomach and liver by an analytical chemist. The defence urged that the poison had been designedly introduced into the body not very long before its disinterment, with the view of fastening the guilt upon the woman, and thus diverting the estate of the deceased into another channel. There was a strong motive for so doing, and also a good opportunity, as the body had been buried in a vault. In the judgment of most of those conversant with the parties, this actually had been

done, and the prosecution felt so little sure of its ground that the trial was abandoned. Impressed with the importance of the subject, Dr. Reese made a series of experiments on the bodies of dogs, injecting strong solutions of arsenic, tartar emetic, and corrosive sublimate into the stomachs of the dead animals, then burying them beneath the ground, and disinterring them at different periods of time, so as to note the difference of result as dependent on the length of time of burial. After twenty-one days' burial in the case of all the three poisonous solutions, on opening the abdomen of the animals the characteristic coloured spots of the respective sulphides were observed on the spleen, the under-surface of the liver, and the portion of the peritoneum posterior to the stomach—yellow in the case of arsenic, orange in that of antimony, and black in that of mercury. Each of these metals was likewise discovered, by chemical analysis, in the liver, spleen, and left kidney, but none in the right kidney. After forty-four days' burial the deposit of the different sulphides was found to be much more decided in all three cases, being noticed on the upper as well as the lower surface of the liver, together with the spleen; also, over the intestines, the omentum, and the kidneys. By chemical analysis, also, the poisons were detected in liver, spleen, and both kidneys. After fifty-nine days' burial the results were found to be very similar to those last detailed, only more decided. Of the three solutions, the arsenical seemed to penetrate more completely than the other two. In reply to the question whether it is possible to distinguish between poison really *absorbed* during life, and that which has been merely *imbibed* after death, the investigator thinks that if, on a careful analysis of the interior of an organ—the liver, for example—with the exterior of the same organ—especially if the examination were made not long after death—the poison were discovered in the interior as well as on the external parts of the organ, the poison must have been absorbed during life, and introduced through the circulation, whereas leakage from the stomach or rectum after death would be discoverable chiefly on the surface of the contiguous viscera. Some recent experiments of M. Scolosuboff, of Moscow, go to show that in dogs and rabbits poisoned by arsenic, this substance is deposited in the brain and spinal marrow in far larger quantities than in other organs. Comparative analyses of equal weights of muscle, liver, brain, and spinal marrow, taken from a dog that had been taking arsenic for five weeks, demonstrated that the brain and spinal marrow contained thirty-six and thirty-seven

times the amount found in the muscles, and nearly four times that discovered in the liver. Although there is no record that these experiments have yet been verified in man, there seems good reason to believe that the statement will hold good in the human subject. Should this prove to be the fact, we shall be in possession of a positive and unequivocal chemical method of distinguishing between *ante mortem* and *post mortem* poisoning by the detection of the poison in the brain and cord, since it is scarcely conceivable that a poison introduced into a body after death could penetrate by imbibition within the cavity of the cranium or spinal column.

THE MIDDLE EAR OF NEONATI IN ITS MEDICO-LEGAL ASPECT.

The following are Dr. Gellé's *resumé* and general conclusions:—

1. In the foetus the middle ear is full of a gelatinous smegma, and contains no air.

2. At the moment of birth this smegma begins to disappear, and in its place air enters the cavity of the tympanum.

3. This is due to the act of respiration and the derivation of blood to the vascular territory opened up by the circulation; the thick reddish smegma grows pale, and is absorbed; it was a body; there now remains but a coating.

4. The auricular cavity is, little by little, filled with air from without. The cries and sucking efforts favour, in their turn, respiration and the aëration of the tympanic cavity.

5. The time necessary to complete this condition depends upon the activity of respiration.

6. When all proceeds well, the transformation takes place in a few moments; rarely it occupies some hours—twelve at most.

On the other hand, if respiration is feeble, if asphyxia, rapid or slow, takes place, the aëration of the tympanic cavity is incomplete—exists only on one side, or not at all. In these cases the contents of the cavity are mixed, the foetal condition being still pretty clearly shown, spite of the ascertained presence of air; it is the combination analogous to that observed in asphyxiated lungs.

Thus, when the examination of the lungs is impossible, or gives uncertain results, the medical expert will be able to find, in the examination of the ear, signs confirmatory or negatory of the child having respired. As possible sources of error, however, it is to be remembered—firstly, that death by hæmorrhage can induce, through anæmia, the artificial production of the cavity of the ear; and, secondly, that aural catarrh often exists at the time of birth, and

that this would not only of itself prove a serious obstacle to the aëration of the tympanic cavity, but that the hypertrophy and hyperplasia of the lining mucous membrane which it would induce, would also oppose the entrance of air. The state of the middle ear proves a most valuable criterion when an examination is made long after the burial, as the smegma will resist decomposition an almost indefinite time.

SIGN OF EARLY PREGNANCY.

In the *American Practitioner* Dr. Eugene C. Gehrung describes a new sign of early pregnancy. It consists in the sensation communicated to the hand through the sound when it touches the ovum:—

“If the sound or probe be introduced into a healthy womb in the direction of its axis previously defined by the usual methods, the sensation communicated to the hand through the sound when touching the fundus is that of touching a moderately solid object, much resembling that produced in touching the roof of the mouth with the same instrument. If an ovum of any size be present, circumstances are changed. The sound will proceed with equal ease through the internal os, but as soon as the ovum is touched the sensation communicated to the hand is like that felt in pushing the sound against a bladder filled with fluid—that is, a gradually increasing resistance—in addition to which, according to the size of the ovum or the amount of pressure exercised, the sound will be driven back, when loosely held, with a greater or lesser amount of force communicated to it by the tendency of the ovum to resume its former globular shape. In other words, the sound meets with an exceedingly elastic body beyond the os internum instead of the solid uterine walls. This symptom, when present alone, is not positive evidence that pregnancy exists, nor, when absent, negative; because there are a number of other conditions which give a similar result. On the other hand, it is a well-known fact that the ovum, in the earlier periods of pregnancy, is only attached to a greater or lesser part of the interior of the womb, and consequently the sound may slip by it without producing that particular effect; yet when found it should caution the operator, and make him reconsider the case carefully before proceeding further.”

It is hardly necessary to call attention to the obvious danger of applying such a means of diagnosis save under exceptional circumstances; in improper hands it would manifestly lead to and facilitate criminal practices.

PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

SAMUEL GORDON, M.D., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, 5th December, 1877.

DR. GORDON, President, in the Chair.

DR. SIGERSON read a paper "On Alternate Paralyses." [It will be found at p. 97.]

DR. MACSWINEY said the paper belonged to a class of communications that it was very desirable to have more frequently submitted to the Society. The subject of electro-therapeutics was engaging great attention, and such a learned essay as they had just heard, dealing with obscure lesions of the nervous system, should be looked on with great interest. Physicians knew how frequently cerebral affections baffled the utmost skill of those who sought to determine their nature, and how often also *post mortem* examinations failed to reveal the lesions that were expected during life. The investigations of Charcot and Vulpian, in France, and of Ferrier and Jackson, in England, pointed out the perfect state of chaos in which cerebral physiology and pathology were at present. Dr. Brown-Séquard's memorable lectures, delivered a year since, were remarkable chiefly for the manner in which that able and learned physiologist employed himself in ruthlessly demolishing the entire structure which had been erected by previous investigations with respect to cerebral and nervous physiology and pathology. That proceeding on his part gave abundant evidence for fearing that the basis on which previous classifications had been built was unstable, uncertain, and unwarranted. However, he did not add to his destructive work any work of reconstruction, so that such of them as were in the habit of attaching importance to the investigations of that distinguished physiologist would have to wait for the new views that it would be correct to

take, in the place of those ancient views which he had so mercilessly destroyed. Coming to particulars, it was satisfactory to find that the author of the paper had pointed out means whereby, if they were closely to follow them, they might be enabled with more certainty to detect the nature of the particular lesion which was present in the various forms of alternate paralysis to which he had called attention. The elaborate communication of the learned essayist was one from which the members of the Society would derive much more advantage by reading it in print than by any attempt to criticise it in detail now.

DR. HENRY KENNEDY said he did not know that the theory with respect to lesions in or about the pons Varolii giving rise to differences of temperature on either one or both sides of the body was tenable. In the present year he (Dr. Kennedy) had had a remarkable case—and he had seen other cases previously—in which the lesion was confined to the neighbourhood of the ventricle, and in which the temperature of the body on the weakened side was constantly below the healthy standard. The disease was entirely confined to the roof of one of the ventricles. In the early stages of the disease the facts with respect to temperature were very remarkable. On the permanently affected side the temperature was from a degree to a degree and a half lower than it was on the sound side. The other point to which he wished to refer was the cause of the disease. He had seen cases very like the one in question in which the disease was distinctly traceable to gout, for many of the paralytic symptoms in those cases entirely disappeared when gout made its appearance. Some years ago Dr. Lynch, of Loughrea, published a case presenting symptoms very analogous to those detailed in Dr. Sigerson's paper, and in which, on the occurrence of gout, the greater number of them disappeared. Therefore, they should be very slow in concluding that those symptoms were always due to disease of the brain. Most serious cerebral lesions might occur in connexion with gout, and nevertheless there might be no reason for supposing that they were caused by any disease of the brain. What was the age of Dr. Sigerson's patient?

DR. SIGERSON—Sixty-five.

DR. WALTER SMITH remarked that Dr. Sigerson had called attention to the valuable aid that Duchenne had shown could be derived from local faradisation as a method of diagnosis in facial paralysis. But in the present state of their knowledge the galvanic excitability of muscles also required to be examined. They knew that in true peripheral facial palsy, as well as in other cases of nerve injury, the total absence of faradic excitability coexisted not only with the preservation of, but with the actual exaltation of, galvanic excitability. It was possible that a further

refinement of diagnosis might be gained in some cases by bearing that fact in mind.

DR. NIXON referred to the case given by Dr. Sigerson, in which, with hemiplegia of the body, accompanied by facial paralysis on the same side, there was inability to close the eye. This fact was interesting, as he (Dr. Nixon) believed that in Dr. Hughlings Jackson's large experience he had not met with a similar case in which the orbicularis palpebrarum was paralysed. With respect to Dr. Duplay's cases, he understood Dr. Sigerson to state that there had been multiple lesion; and, if so, the paralysis of the muscle might be accounted for by pressure on the nerve.

DR. SIGERSON, in reply, said the subject of the correlation of lesions and temperature was one into which he had avoided entering. It was opened up by the quotation he had given from Dr. Brown-Séquard, but he (Dr. Sigerson) had not sufficient data to enable him to proceed further. Neither could he give any opinion as to the probability of gout being a predisposing cause of the affections in question. He had very carefully tabulated all the home and foreign cases which he had found recorded, and gout was not mentioned in any of them. He admitted that it was possible—according to Dr. Kennedy's experience—that gout might be present; but having taken all possible precautions in the case which he had submitted, he (Dr. Sigerson) was as certain as it was possible to be that the lesion was in the pons Varolii, and he was almost as certain that the cause was a blood clot. It was possible that gout might have helped the formation of that lesion. He was aware of the fact stated by Dr. Smith of the different effects of faradism and galvanism on muscular irritability. Indeed, he believed that Dr. Duchenne had first called attention to the fact that, along with diminution or even annihilation of faradic excitability, there might exist exaltation of galvano-muscular excitability. Rosenthal had referred to the fact as of use in cases of diagnosis; but though he was prepared to admit with Dr. Smith that this afforded some further means of diagnosis in certain cases of paralysis, he did not think that in the present case it was necessary. He trusted that Dr. Walter Smith would work out the idea he had referred to. There was great probability that some further step in their knowledge might be accomplished in this direction. As to the question of the paralysis of the orbicularis palpebrarum, alluded to by Dr. Nixon, it was perhaps due to the length of the paper read that Dr. Nixon omitted to observe that Dr. Duplay's cases were not cases in which multiple lesions were discovered. Dr. Duplay had carefully examined the matter, as far as he (Dr. Sigerson) could ascertain, and had found that in aged persons there was a lesion of the cerebrum which could be correlated with paralysis of the orbicularis palpebrarum. In that case, of course, there

was no multiple lesion, or at least no lesion pressing on the facial nerve, which would have been mentioned if it had occurred. With regard to the case of the boy who had been under Dr. Sigerson's care, there was paralysis of the orbicularis, and at the same time conservation of muscular contractility, which pointed to a lesion above the point of decussation.

The Society then adjourned.

Wednesday, January 2, 1878.

DR. GORDON, President, in the Chair.

DR. WALTER SMITH read a paper on "Facial Paralysis, with Remarks on the Action of Induced Currents and Interrupted Voltaic Currents." [It will be found at p. 125.]

The PRESIDENT complimented Dr. Smith on his practical paper. He had known facial paralysis of a transient character arise from deficient circulation. Such cases did not last more than two or three days, and were generally confined to the region supplied by the portio dura nerve.

DR. HENRY KENNEDY observed that in one case of facial paralysis which had occurred in his practice he had found the treatment by mercury pushed to salivation successful. In that case electricity had been tried and had failed, but it was at a time when its application was not so well understood as now. He had seen several cases of transient paralysis, such as the President had alluded to. They were accompanied with syncope, and did not last more than a few hours, and were probably connected more or less with deficient circulation.

DR. AQUILLA SMITH said he had treated half a dozen cases of paralysis with mercury, and the treatment failed in every one of them. They were all afterwards successfully treated by electricity.

MR. H. G. CROLY said the last of the cases mentioned by Dr. Walter Smith had been under his care. Very early in the case he felt a small gland in the region of the stylo-mastoid foramen. Whether it pressed on the nerve, or came on from the irritation of a blister which he had applied, was hard to say. The case was almost well now, the child being able to shut her eye, whistle, and wrinkle her brow; but there was a slight appearance of paralysis remaining. He thought the delay between the early treatment of leeching, &c., and the proper application of electricity by Dr. W. Smith had been too long delayed in this case, and that a certain amount of muscular atrophy had been allowed to

come on. If Dr. Smith had enjoyed an opportunity of applying electricity from the first, he was sure the child would have been well by that time. He alluded to some cases in which facial paralysis had been induced in his practice as the necessary result of surgical operations, and asked if Dr. Smith had any experience of injecting minute doses of strychnine in facial palsy of the portio dura nerve.

DR. FINNY asked what interval should elapse between onset of the palsy and the application of faradic or galvanic electricity? Dr. Smith's cases left them in doubt as to whether, in spite of the existence of galvanic excitability, the cases would recover rapidly or not.

DR. WALTER SMITH, in reply, said he had tried strychnia in several cases of facial paralysis, and had convinced himself that in no case was any improvement due to its use. He believed that the sooner electrical treatment was begun and carefully carried out the better would it be for the patient. A prognosis could not be derived from a single examination, but must be the result of several examinations made within short intervals of each other.

A Case of Typhus with Hyperpyrexia. By JOHN WILLIAM MOORE, M.D., Univ. Dubl.; Fellow K.Q.C.P.; Physician to the Meath and Cork-street (Fever) Hospitals.

THE present "Clinical Record" is interesting chiefly on account of the exceptionally high temperature observed during the death-agony of the patient, who succumbed on the nineteenth day of a typhus fever of the nervous or ataxic variety.

The following is a brief history of the case :—

Margaret T., aged thirty-eight years, married to a shoemaker, and residing in Cuffe-street, Dublin, was admitted to the Meath Hospital on the afternoon of Wednesday, September 12th, 1877. It is worth noting that a young man from the same house was under treatment for severe typhus in Cork-street (Fever) Hospital from July 18 to September 8. There was no special medical history previously to her present illness, which commenced on Tuesday, September 4th, 1877, with a shivering fit, followed by chilliness so great as to oblige her to hang over the fire the greater part of the day. She also suffered from pains in the limbs. She fought against her illness until the following Friday (4th day), when she took to bed, as she was getting much worse. On Saturday, severe pain in the small of the back, and, next day, vomiting of "watery stuff" were superadded to obstinate constipation, which was an early symptom. From the outset also she was very sleepless. She did not come into hospital until the afternoon of the ninth day. In the evening her pulse

was 120, her respirations were 28 per minute, and the axillary temperature was 104·9°.

Next morning her face presented a dusky, congested appearance; the eyes were heavy, and the conjunctivæ deeply suffused; her tongue was heavily furred, but moist; both it and the lips were exceedingly tremulous. There was a marked "nervousness" of manner. A profuse mulberry eruption covered nearly all parts of the body—the original maculæ had become true petechiæ, which were unusually distinct and dark in colour. Her pulse was 124, not strong; respirations, 24; temperature, 104·3° (at 10 a.m.). The heart's action was rapid and weak, but both sounds were audible, and the impulse could be felt in the normal situation.

Treatment.—It was decided to free the bowels by a simple enema of olive oil and warm water, to support the strength by strong beef-tea and abundance of milk, four ounces of port wine, and moderate doses of quinine.

September 14th (11th day).—The enema acted twice, bringing away dark, solid fæces in considerable quantity. There was a slight remission of fever, the morning temperature being 103°.

September 15th (12th day).—She spent a sleepless night, being very restless and moaning constantly. The heart was becoming rapidly weaker—the impulse was feeble, the first sound indistinct, the second relatively strong—being thrown into relief by the subsidence of the systolic sound. Strychnia and digitalis were added to the quinine mixture; and the port wine, which had been increased to 8 ounces the previous day, was again increased to 12 ounces, with 4 ounces of whiskey. The heart's action was impeded by tympanites, to combat which a turpentine enema was administered. It acted once, with some relief to the patient. The persistent sleeplessness and increasing head symptoms led me to try, cautiously, tartar emetic and opium, as recommended by Dr. Graves. Only two doses, however, were taken—too small a quantity to induce sleep on the one hand, or to be accused of injuring the patient on the other. One sixth of a grain of tartarated antimony and 10 minims of tincture of opium alone were given.

September 16th (13th day).—She passed another sleepless night, with much muttering delirium. The tongue was moist; but there was progressive failure of the heart. Its action was very weak—quite fœtal in character. Port wine, ℥xii.; whiskey, ℥vi. Towards evening the temperature began to fall with an attempt at crisis. At 7.30 p.m. there were marked cerebral symptoms; but in the night the bowels moved, and both diaphoresis and diuresis occurred, so that an evident effort at crisis was made.

September 17th (14th day).—The morning temperature was moderate—102°; but this one favourable symptom was accompanied by a rise in the rate of both pulse and respirations—a circumstance of evil omen. The

rash was on this day evidently fading from the anterior aspect of the body, although it remained very petechial even in this situation.

September 18th (15th day).—The temporary remission of pyrexia was now succeeded by an exacerbation in the symptoms—a morning temperature of 103.7° , constipation, commencing retention of urine, and persistence of the rash. The increased meteorism obliged us to order another turpentine enema, which operated once.

September 19th (16th day).—Another pseudo-crisis occurred on this day, with *profuse diaphoresis*, and a fall of temperature to 101.7° . Again there was a complete want of accordance in the symptoms—the respirations remained as fast as ever (48 per minute), the pulse fell only four beats per minute, there was complete retention of urine. The pulse could scarcely be counted, owing, first, to its weakness; secondly, to the intensity of the subsultus which was present. The urine was now slightly albuminous, and the urea was partially decomposed—the fresh urine, drawn off by catheter, effervescing briskly on the addition of dilute nitric acid.

September 20th (17th day).—In no way better, in some respects worse; temperature rising; bowels costive, with tympanites.

September 21st (18th day).—Morning temperature, 104.8° ; pulse, 122 to 126; bowels have not acted; no return of power over the bladder; the urine is drawn off by catheter every eight hours or oftener; a turpentine enema ordered, and to omit the 3-minim doses of liquor strychniæ which she has been taking. In the evening a third attempt at crisis reduced the temperature to 103.2° , but the pulse remained unaffected.

September 22nd (19th day).—Much worse in every particular; unconscious, with coma vigil; pupils contracted and sluggish to stimulus of light; considerable albuminuria; morning temperature, 106.4° . Ordered: one grain doses of camphor and musk every second hour. At 6.45 p.m. I visited her again. She was bathed in a cold perspiration; the extremities were cold, her eyes fixed, with coma vigil; bronchial râles heard universally over the chest; axillary temperature (taken twice with two reliable thermometers) was 108.6° . She was manifestly dying. At 7.30 p.m. the last observation on the temperature was made; it was found to be 109.1° . Three hours later she was dead. Unfortunately no observation was made after death. This was partly owing to the late hour at which she died.

Authenticated temperatures as high as those noted in this instance—namely, 108.6° and 109.1° —are so rarely recorded that I was induced to bring forward the foregoing “Clinical Record.” So far as I am aware, no such temperatures have been observed in the Meath Hospital since the modern introduction of Medical Thermometry. In a valuable appendix to Dr. Stokes’ work on Fever, my colleague, Dr. Arthur

Wynne Foot, gives a *résumé* of thermometrical observations in the medical wards of that hospital during the three years, 1871, 1872, and 1873. Among 9,248 observations, the highest reading noted was 107·2°. He says:—"On 27 occasions temperatures of 105° Fahr. or upwards were registered in typhoid fever in 15 patients, and of the 15 patients in whom the temperature on one or more occasions reached 105° Fahr. or upwards, five died." The highest temperature recorded was in "a girl aged 16; temperature on 30th morning 107·2° Fahr.; died on the 31st evening. Her mean temperature (51 observations) during the twenty-six days she was in hospital was 103·1° Fahr. The morning temperature, 107·2°, was coincident with severe rigors, preceded by violent pain in the abdomen, ushering in peritonitis, not due to perforation, but to propagation outwards of the irritation arising from numerous and extensive ulcerations of the intestinal glands."

Dr. Murchison,^a speaking of the temperature in typhus, observes that "a severe case is often characterised, not merely by a high temperature in the first week, but by an anomalous or irregular range in the second; for example, *by an absence of the morning fall, or by a sudden fall with a rise of pulse, or with no improvement in the general symptoms. In fatal cases there is usually a rise of two or more degrees just before death or in the death-agony.*"

I have italicised the latter part of this passage, as it bears so directly on the present case, and is so fully illustrated by it.

Wunderlich^b remarks that "*Fatal Cases* of exanthematic typhus generally announce themselves even from the very beginning by the enormous height of the temperature (41·2° C.=106·16° Fahr., and even more). The transient remission at the end of the first week is wanting in these cases. Death may occur in the second week with continual high temperatures. If the case enters the third week, *some remission may show itself on the fourteenth day, but this must not be regarded as a favourable symptom, and is very soon compensated.*

"Yet even in fatal cases *the temperatures in the third week are not so high as at the earlier periods, at least till near the death-agony.* The daily maxima do not exceed 40·8° C. (105·44° F.), but are, for the most part, moderate. The danger to life during this third week is indicated not by the height of the fever, but by its continuance.

"*Just before death, and in the death-agony, the temperature constantly rises in exanthematic typhus.* In all my cases in which it was possible to make observations, there was a rise of temperature during the agony of at least 1·25° C.=2·2° F., in one case of even 3·6° (6·48°); and on an average about 1·8° C. (3·24° F.). During the agony the temperature

^a The Continued Fevers of Great Britain. Second Edition. Page 137.

^b Medical Thermometry. Translated by W. Bathurst Woodman, M.D. The New Sydenham Society. 1871. Page 331.

was seldom so low as 40° (104° F.); more usually it was about 41° — 42° C. (105.8° — 107.6° F.), and once 43° C. (109.4° F.).”

The curves of temperature in this last case are given in Table IV. at the end of Wunderlich’s book, and are also to be found in Diagram VII. in Murchison’s work, where the case is described as one of “*Typhus, fatal on 17th day, with very high temperature (109.4° F.) before death.*” The chart is strikingly like that which is the subject of this paper.

The dread significance of such temperatures is aptly expressed by Wunderlich in his definition of “Hyperpyretic Temperatures”^a—those “which in every known disease, except relapsing fever, in all probability indicate a fatal termination— 107.6° F. (42° C) or more.”

To attempt an explanation of this hyperpyrexia in disease would be foreign to the scope of this communication, which is simply a clinical record. Nor do I in any sense feel competent to undertake a task which, up to the present, has apparently foiled even the ablest thinkers in the domains of practical medicine, of morbid anatomy, and of physiology. As Wunderlich well observes—“In the foreground is the question: ‘On what does the abnormal temperature depend?’ But this is by no means identical with the question: ‘What constitutes fever?’”

It will serve my present purpose to direct attention to an admirable résumé of modern “Theories of Fever” in Wunderlich’s work (pages 184, *et seq.*); to Dr. Burdon Sanderson’s Monograph on the Process of Fever, in *The Practitioner* for April, May, and June, 1876;^b and to an original communication in the *Medical Times and Gazette* for 1877 (Volume I.), by Dr. C. Handfield Jones, on “Two Cases of Fever with Hyperpyrexia.” The last-named author holds “that there exists a nerve-centre, or set of nerve centres, situated in the upper part of the cervical cord, or in its vicinity, which in health controls the development of heat, probably by lessening the combustion of protoplasm; but which, under the influence of certain poisons, or heat, or exhaustion, or direct or remote injury, loses this faculty, and that this loss is the essence of fever.” In proving his case, Dr. Handfield Jones^c notices the occurrence of hyperpyrexia in cases of rheumatic, typhus, typhoid, and scarlet fevers. “The symptom,” he says, “is commonly associated with low delirium, stupor, or coma. These might be regarded as the results of the overheated blood acting injuriously on the cerebral tissue. But, as instances of relapsing fever and Mr. Teale’s case show that very high temperature does not necessarily produce these symptoms, it seems more reasonable to regard them as co-effects of the hyperpyrexia, all being of the nature of paralyses, and all being produced alike by the original cause of the fever. High

^a Loc. cit. Page 7.

^b Reprinted, by permission, from the Appendix to the Report of the Medical Officer of the Privy Council on Scientific Investigations. New Series. No. VI.

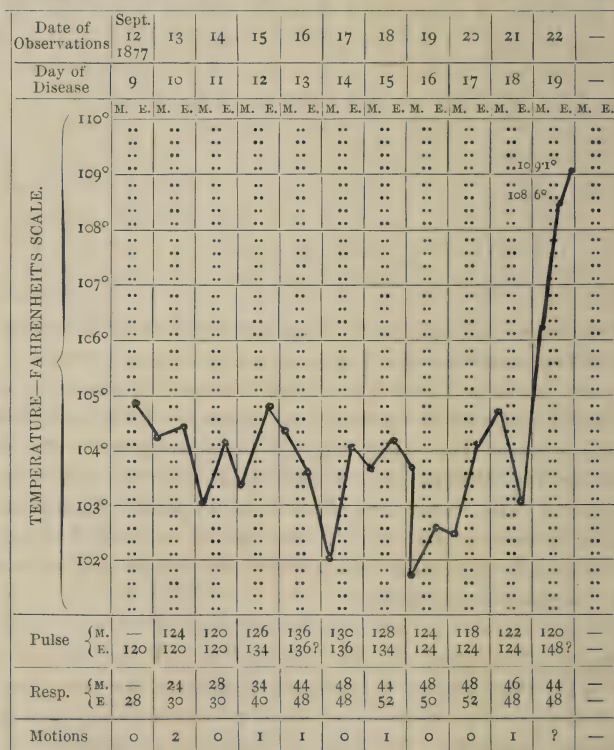
^c Loc. cit. Page 569.

temperature is, I believe, most prone to occur in cases of the greatest gravity. In some fatal cases of scarlet fever Dr. B. Woodman has noted a temperature of 115° . The nervous system is so constantly and injuriously affected by the fever-poison in all severe instances of the above maladies that it is no more than might have been expected that the heat-regulating centre should occasionally be paralysed."

Such is the neuro-pathological view of fever. Shall we further believe, with Dr. Burdon Sanderson, that the mechanism, by which the influence of this "*schützendes Etwas*" (as he more indefinitely describes the Law of Temperature) is exercised in restraining temperature, is *not* the circulation?

CLINICAL CHART OF TEMPERATURE, &c.

M. T.; Age, 38; Disease, *Typhus Fever*; Result, *Death on 19th day.*



The PRESIDENT said the case reported by Dr. Moore was remarkable on account of the three efforts at crisis which had occurred in the course of it.

The Society then adjourned.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

FORTIETH ANNUAL SESSION.

Saturday, 8th December, 1877.

MR. DARBY, President, in the Chair.

Specimen of Fibrous Polypus.

DR. M'CLINTOCK.—This is a small but admirable specimen of fibrous polypus, which Dr. Torney and I removed from a patient of his on the 15th of this month. The only interest attaching to it is that the lady from whom this was removed, who is about forty-five years of age, and has given birth to three or four children, was the subject of a similar complaint about four years ago, when Dr. Torney and I removed from her a polypus of the same nature as this, and of the same size. On each occasion the polypus had a well-defined pedicle, and had passed almost completely behind the os uteri, so that there was no difficulty in the operation. It was the first case in my experience in which I had, in the same patient, to remove a second polypus that had passed into the vagina.

THE PRESIDENT.—I removed two polypi from the anterior lip of the uterus. When removing one I discovered the other. The one I first removed was the largest, and after I had removed it the uterus receded, and I found a difficulty in catching hold of the small one, which was not as large as a pea. The other was not quite as large as the one now exhibited by Dr. M'Clintock. I had to wait a few weeks until the second grew a little larger, and then I removed it. Should, however, such a case be presented to me on any future occasion, I shall remove the smaller tumour first, and subsequently the larger one, so as to complete the operation at one sitting.

The Influence of the Uterus in Eye Disease. By H. R. SWANZY, M.B., F.R.C.S.I.; Professor of Ophthalmic and Aural Surgery, Royal College of Surgeons in Ireland; Surgeon to the National Eye and Ear Infirmary; Ophthalmic Surgeon, Dr. Stevens' Hospital.

ONE of the greatest efforts of modern ophthalmology has been directed towards bringing this branch of medical science into intimate relation with the others. At the present day, indeed, there are few eye diseases which we recognise as purely local affections. While they require

special local treatment, most of them demand attention also to some constitutional defect, or some disease situated perhaps in a distant organ. Thus we know that eye diseases may have their starting-point in the heart, kidneys, or spleen. The visual organs may be affected in tuberculosis; they often suffer in diseases of the nervous and vascular systems; and I need hardly say how frequently they become diseased in syphilis and struma. This evening, by the kind permission of the Council of the Society, I purpose to occupy your attention for a short time in considering the influence of the uterus in eye disease.

At the very outset I must explain that the subject is by no means a completely developed one. It is a very wide one, and its investigation is surrounded by many difficulties. Not the least of these is the circumstance that very few ophthalmologists are at the same time experienced in gynæcology, and that it is also rare to find a gynæcologist who knows anything of ophthalmology. Still the influence of the uterus in eye disease is every day attracting more attention amongst oculists, and we may hope that before many years have gone by we will be in possession of a number of well-authenticated facts which will teach us how far this causal relation between diseases of the uterus and the eye exists, and will enable us to understand in what way it comes about. At present we possess some facts only, while much of the subject remains hypothetical. If we could enlist the interest of gynæcologists in the matter much more rapid progress would be made, and it is partly with this object that I come before you to-night. In what I am about to say I shall not confine myself to my own experience for material, but I shall draw on the observations of others also, in order to present you with as complete a view of the question in its present stage as is compatible with the time at our disposal.

The first disease which I shall bring under your notice, as probably having its primary cause in the uterus, is iritis occurring in young girls from about the eleventh to the seventeenth year of age—say within a period varying from two or three years prior to the establishment of menstruation up to two or three years after they commence to menstruate. I am much interested in these cases, because, so far as I am aware, they form a point of connexion between the uterus and the eye which has not before been pointed out. I am aware, however, that others besides myself have observed this connexion, for while lately in Paris I learned in conversation with a distinguished oculist there that his experience had led him to form a similar view on the subject. The whole number of these cases which I have seen is seven. It may seem few, but the smallness of the number is accounted for by the fact that I have not seen a single case of the kind in hospital practice. These patients seem always to enjoy good general health, and so far as I have been able to ascertain they did not suffer from any serious irregularity in the uterine

functions, should that organ have come to maturity. The most I have detected has been a somewhat insufficient menstrual flow. I am unable therefore to connect the disease of the eye positively with a disease of the uterus, but I am inclined to regard the uterus in some way as the starting-point of the iritis, for these reasons:—(1) Iritis is extremely rare at such an early time of life, unless as dependent on congenital syphilis, or as secondary to corneal diseases; (2) I have never seen a case similar to those I speak of in the male; and therefore (3), when one finds it to occur with a certain frequency at the time of life when the uterus is approaching maturity or has lately reached it, and when one is able to exclude all other causes, the inference is a fair one that the uterus in some way has given rise to the iritis.

The form of iritis is similar in all these cases; there is usually little or no pain, and but little vascular injection of the eye or photophobia. The anterior chamber usually remains clear, and there is no deposit on the posterior surface of the cornea. There is a great tendency to the formation of posterior synechiæ, and the stroma of the iris in bad cases becomes indistinct and discoloured. The vitreous humour is very liable to become cloudy, showing that the cases are not ones of simple iritis, but that the ciliary body and choroid are implicated. Perhaps, indeed, it would be more correct to describe these as cases of irido-choroiditis. In some of the cases I have seen it was the dimness of sight caused by these opacities of the vitreous which first induced the patient or her parents to seek advice. The affection is very slow in its progress, very difficult of cure, and very liable to recur.

One of the most serious cases of this kind which I have observed is at present under my care. I shall not impose the whole history of the case upon the Society, but shall briefly refer to its principal features.

On 5th July last I was consulted by the parents of a particularly healthy-looking young lady, aged sixteen, on account of her defective vision. In the right eye there was the form of irido-choroiditis which I have described, and in the left eye there was optic neuritis. The sight of the right eye was reduced to the power of counting fingers at 8 feet; but neither the patient nor her friends were aware of any defect in the functions of the left eye, the one with optic neuritis, although its vision was only a little more than one-third of what it should have been. She had first complained of her sight about two months before I saw her. Five months previously the menses had remained absent on one occasion, and at that time she had a bleeding from the nose. Since then the menstruation had been regular as to time, but the mother seemed to think the flow not quite as abundant as is usual in girls of her age and of such full habit. She was particularly subject to headaches, but it had not been remarked that they were more severe at or about the menstrual period. Since she has been under my care, I think I have noticed that

the headaches are more severe at that time. After remaining in town for six weeks the patient had so far recovered as to be able to return home. She had not been at home for more than ten days, when, while stretching up to nail a picture on the wall, she found the sight of the left eye suddenly become dim. She was at once brought back to me, and the ophthalmoscope displayed an enormous hæmorrhage situated probably between the retina and choroid. The right eye had also relapsed, the vitreous humour being very much more clouded than when I had last seen her. At present the sight of the eye in which the hæmorrhage occurred is extremely imperfect, owing to a detachment of the retina which has resulted. The sight of the right eye is tolerably good, and if I can save it for her I shall think myself extremely fortunate in the case. We have in this case three diseases of the eye—irido-choroiditis, optic neuritis, and intra-ocular hæmorrhage. It was to give an example of the first of these that I brought forward the case in this part of my paper; but the two other affections are also recognised as sometimes the consequence of disordered uterine functions, as I shall show further on. Fortunately it is a rare thing to find them all combined to destroy vision as in this instance, especially in so young a person.

The treatment which I have usually adopted in these cases of iritis has been chiefly local during the acute stage of the inflammation—atropine, warm fomentations, &c. After the acute inflammation had subsided I have given tonics, especially iron. In the case I have just alluded to the treatment was very various according as the symptoms demanded. For a long time, although I was fully convinced of the connexion between the eye trouble and an imperfect menstruation, yet treatment could be only partially directed towards the uterus, owing to the necessity for promoting absorption of the intraocular hæmorrhage by a horizontal position for several weeks. Such confinement to the house is not, I conceive, well calculated to improve the uterine functions. However, aloetic purgatives were administered with bromide of potassium. Latterly iron has been given in place of the bromide. On the whole, the menstrual flow is now more abundant; but that this has had any great influence in producing restoration of a healthy condition within the eyeballs, I am unable to say. The most that I can believe to have been accomplished by the uterine treatment, if I may so call it, is that no very serious hæmorrhage occurred again, nor any fresh attack of iritis.

Inflammation of the optic nerve and retina may depend on disturbances of menstruation. In the *Irish Hospital Gazette* for 1873 (p. 46) I published a case of neuro-retinitis in a girl, aged nineteen, whose menstruation was sparse and painful, and in whom the eye affection always became aggravated at the monthly periods. I observed another similar case, of which unfortunately I have lost the notes. Mandelstam^a has

^a Klin. Beobacht. a. d. Augenheilanstalt zu Wiesbaden. 1866.

seen many cases of optic neuritis where marked menstrual disturbances had gone before. Von Graefe^a recognised the existence of such a connexion. Mooren^b has seen cases of neuro-retinitis after suppression of menstruation, and he is of opinion that retroflexions of the uterus and ovarian tumours may give rise to the same affection.

Retinal apoplexies are sometimes the consequence of cessation or suppression of the menses. Thus Liebreich, in his "Atlas of Ophthalmoscopy," gives a drawing of a retinal hæmorrhage which occurred in a woman forty-five years of age, in whom the menstruation ceased, and who had had it very abundantly up to that time. He mentions that he had several times observed the same ophthalmoscopic appearances under similar circumstances. Samelsohn^c describes a case of absolute amaurosis after a sudden suppression of the menstruation in a girl, aged twenty-one years. There were no ophthalmoscopic appearances, and the probable diagnosis consisted in a hæmorrhage into the optic nerve behind the globe. The patient was working in a cold stream during her menstrual period. The menses ceased at once, and on the evening of the same day vision began to be affected. In the course of five days the patient became stone blind. The treatment consisted in warm foot baths, mustard poultices on the upper part of the thighs, emetics, and the application of Heurteloup's artificial leech to the temples. Later aloetic and iron pills were given. In twenty-four hours the sight began to recover, and in the course of eleven days the vision was again almost normal. Seven weeks after the sudden interruption of the menstruation it returned, and simultaneously the pains in the breasts disappeared, with which she had been troubled since the loss of her sight.

Atrophy of the optic nerve has been noted repeatedly by Pagenstecher^d as occurring in women who had suffered from severe menstrual disturbances (irregularity, early cessation), which he regarded as the cause of the eye disease.

I have now to speak of an affection of sight connected with uterine disorder which until lately has been classed among eye diseases, but which we now know to be nothing more than a symptom of a certain uterine disease. It is a form of asthenopia to which the name of *Kopiopia Hysterica* has been given. I have for some time been acquainted with this affection, but, like the iritis of which I have spoken, I have never seen it except in private practice. One of the first patients who consulted me was a case of this kind. I was extremely puzzled by it, never having seen anything of the sort before. Since then I have seen a considerable number of such cases, but only quite lately have I read the

^a Archiv. f. Ophthal. XII. 2.

^b Ophthalm. Mittheilungen. 1874.

^c Berl. Klin. Wochenschr. 1875. P. 27.

^d Klin. Beobacht. 1866.

first description of the affection, along with an explanation of its cause. It has been described at length by Prof. Förster, in his article in Graefe and Sæmisch's new Handbook of Ophthalmology, and the pathological conditions of the uterine apparatus which Prof. Freund, of Breslau, has invariably found accompanying it are cited by Prof. Förster. This article has been published within the current year (1877).

Patients suffering from the affection complain chiefly of inability to read, write, sew, or use their eyes for any near work, owing to darting pains through the organs when they attempt to do so. The pains in and about the eyes, however, although increased by any use of them, are also present with greater or less severity at other times, and are intensified by anything which depresses the general tone of the patient, such as want of sleep, fatigue, sorrow, &c. The patients are troubled with photophobia, especially at night, when the lamps or candles are lighted. Indeed, many of them can bear the direct sunlight without inconvenience, when even a shaded lamp in the room in the evening will give them great distress. No organic disease of the eyes is present. Some hypermetropia or insufficiency of the internal recti may be found, and is likely enough for a time to mislead the surgeon. The eyes are never injected; there is no swelling of the lids; no epiphora. Prof. Förster has remarked that the patients have good days and bad days, but that there is no regular interval between these variations, and he has also observed that they are never kept awake by the pain of their eyes; and he says that shortly before and during menstruation the symptoms are usually more severe. These are points which I have not noted myself. One generally finds these patients endowed with the greatest volubility, the torrent of words with which they describe their ailment affording in itself a symptom of the disease. Very often they enter into the most curious details concerning their trouble. Thus one young lady assured me that there was only one book which she could read without suffering intense pain, and that was one particular copy of the Bible. She could read it for any length of time without the slightest inconvenience, but not even another copy of the same edition could she look at. I was told that there was nothing peculiar about this book; it was of the ordinary hand size. One of Förster's patients was compelled always to shut her eyes when passing through a doorway, as the draught brought on the pain, and another got a pain in her stomach whenever she attempted to read. Although in other particulars these patients might be termed "nervous," still they do not usually suffer from any marked hysterical symptoms apart from their eyes.

Prof. Freund, of Breslau, has found by means of a large number of *post mortem* examinations of women who had complained of these eye symptoms that they were uniformly affected with a certain uterine disease, which he claims to be the first to have recognised. It is a chronic inflam-

matory process, attacking the parametrium, and producing in bad cases atrophy of that tissue, with displacement of the pelvic viscera. It is liable to affect married and unmarried women at all times of life. Its course is a very chronic one, extending often over a period of several years, and treatment has little effect upon it. Painful regions may be sometimes found in the lateral *cul de sac* of the vagina, in the course of the broad ligament, and stretching towards the pelvic walls. There is often venous congestion of the vagina, with chronic metritis, catarrh, and ulceration. In less severe cases these local signs may be but slightly marked, and the recovery may take place in a comparatively short time. In none of the cases which have come under my care have I noted any of these pelvic signs; but I must acknowledge that I have seen but two or three rather imperfectly marked cases since I became aware of the nature of the affection. The best treatment consists, I think, in doing as little as possible in the way of medicine, but by attention to the general health to promote the cure, and by diverting the patient's attention from themselves to alleviate the symptoms. For the latter purpose there is perhaps nothing so useful as change of scene or foreign travel. Probably a foreign chalybeate spring would combine everything that can be desired in the treatment.

In the foregoing I have referred to five affections of the eye which seem to depend sometimes on uterine derangement—a form of iritis, neuro-retinitis, apoplexies of the retina and optic nerve, atrophy of the optic nerve, and an affection called *kopiopia hysterica*. It is more than probable that this list might be considerably extended, but we must be careful not to fall into the error of regarding every eye disease which happens to be concomitant with a derangement of the uterine functions as dependent upon the latter.

There are just now two girls in the National Eye and Ear Infirmary suffering from different eye diseases, each of them peculiar in its form. Both of these patients are very irregular in their menstruation, still I do not feel sure as to a connexion between this and their eye troubles.

DR. KIDD.—The Society is greatly indebted to Mr. Swanzy for his paper, which is one of extreme importance. It illustrates the necessity of medical men not altogether confining themselves to special studies, but—while they must be mainly devoted to special subjects—of keeping themselves *au courant* with other branches of medical science and literature. If for nothing else, it is very important that we should carefully consider this paper. I myself know nothing of ophthalmic surgery, but through the kindness of my friends who do, I have had patients sent to me with the intimation that they had a condition of the eye that depended on uterine disease. I found in most of those cases that as the uterine disease improved, the disease in the eyes improved also. In very many

instances I have seen the connexion between uterine disease and the condition of the eyes which has been described by Dr. Swanzy. I have specially noticed cases in which vision has been impaired, and chronic uterine hæmorrhage at the same time going on. This day I saw a lady who has a large fibrous tumour, and who has suffered very much for many years from hæmorrhage. She has nearly lost the sight of her right eye; and Dr. Jacob, whom she consulted with regard to her vision, has informed me that the vitreous humour of her right eye is quite opaque. A gentleman, who is not a professed ophthalmologist, thought she had cataract, but Dr. Jacob says it is not. I have seen many cases in which, while the hæmorrhage was going on, the patients suffered very severely in the eyes. We know the anxiety as to their sight that women have during their puerperal state. They are aware, perhaps instinctively—or at all events it is a matter of common knowledge—that during that condition their eyes are very delicate, and very easily injured by use; and they are careful to avoid reading or otherwise using their eyes while that condition lasts. The eyes are, I believe, often injured by reading too soon after confinement.

DR. MACAN.—When the state of the uterus during pregnancy is sometimes accompanied by loss of sight, it is easy to conceive that lesser affections of the uterus may also lead to less severe disturbance of vision. Dr. Churchill gives cases in which the sight was lost during pregnancy. These are, I think, quite distinct from cases of loss of sight from albuminuria dependent on pregnancy. In all the cases, but one, vision was completely restored after delivery.

DR. MACSWINEY.—We should not omit to notice one of the practical results of the paper which has been read—I mean the treatment to be adopted in affections of the eye which spring from some peculiar conditions of the uterus as distinguished from that which is employed when the affection of the sight arises from local disease of the organ itself. The paper has a valuable bearing in this respect. Obviously, as has been indicated by the learned reporter, the treatment must be totally different in such cases as he details from that which would be proper when there is acute affection of the visual organs arising from other causes. While I am not aware that there has been previously, in this country at least, any formal recognition of morbid relations between uterine affections and affections of the eye, I am well aware that hospital physicians have long since recognised that such relations do exist; and I think that every practising physician must have met with cases in which it was clear that affections of the sight complained of by young females, who were, for the most part, what physicians are in the habit of calling hysterical, were plainly referable to some disturbance of the uterus. The

treatment adopted in those cases—and I speak both from the experience of others and from my own—indicated quite clearly that such a connexion was recognised; but it has been customary with physicians to refer those cases ultimately to the care of ophthalmic surgeons, who, being specialists, would discriminate more distinctly and in more detail the particular lesions of the eye present in the particular instance.

DR. HENRY KENNEDY.—I saw some years since, in Guy's Hospital Reports, a valuable paper, in which cases were related of not merely the eye, but of the other senses also being affected, and the affections being connected with pregnancy, and recovery taking place subsequently. Cases were given of loss of both sight and hearing from that cause. As to what has fallen from Dr. MacSwiney, every physician is perfectly aware of, and must have met with cases of loss of sight from affections of the uterus. As a general rule in those cases, the pupils of the eyes are much dilated, and the loss of vision is great, and I have known it to be even permanent. The treatment in such cases is always of a tonic character. I do not at all speak of the interior state of the eye; but dilated pupils and derangement of vision are very common in hysterical cases.

THE PRESIDENT.—That is the very point in which Dr. Swanzy's paper is valuable, for it points to uterine disease not merely as creating or initiating functional disease of the eyes; but such organic disease as Dr. Swanzy has spoken of as arising from diseases of the womb is, I confess, new to me.

DR. SWANZY (in reply).—I have to thank gentlemen for the kindness with which they have spoken of my paper. I only intended it to be suggestive. It does not pretend to definitely settle anything on the subject. I hardly think that the experience of any one man would suffice to find out all that will have to be found out on the subject. A far more extensive field of observation than I have obtained is necessary to arrive at certainty. It is possible, as has been said, that a great many cases of affection of the eyes are traceable to a deteriorated state of general health produced by the affection of the uterus. With regard to the case of chronic uterine hæmorrhage, and accompanying opacity in the vitreous humour of one eye, mentioned by Dr. Kidd, I would be inclined to explain that opacity by supposing that it was due simply to a hæmorrhage into the vitreous humour. I happened lately to have been in communication with a distinguished London oculist in reference to a case of the same kind. There were hæmorrhages into the vitreous humour, but in that particular case no uterine hæmorrhage; nevertheless he told me he had often come across similar conditions of the eye in

which the patient did suffer from chronic hæmorrhages. He called the case one of hæmophilia. Dr. Kidd also referred to the fact that the eyes of women in the puerperal state are often affected, and other speakers spoke of pregnancy as giving rise to defects of sight. These defects of sight usually depend on one of two things—viz., either (as Dr. Macan has mentioned) albuminuria, with the particular changes in the retina which albuminuria of every kind produces, including fatty degeneration of the retina, with hæmorrhages and optic neuritis; or on paralysis of the power of accommodation of the eye. In cases of fever or severe illness of any other kind which produces a lowering effect, the power of accommodation of the eye breaks down, and accordingly when it has to be exerted to a more than usual degree, the patient is unable to read or to see near objects. That I imagine to be the common explanation of hypermetropia in women pregnant or after child-birth. I agree with the remarks of the President that the point most to be attended to in my paper is that relating to the organic changes that may take place in the eye as the result of uterine disorders.

DR. ATTHILL read a paper on "*Post Partum Hæmorrhage*," which will be found at page 60 of the January number of this Journal.

DR. M'CLINTOCK.—Was the stream of hot water conducted directly into the uterine cavity?

DR. ATTHILL.—In both cases I passed the fingers of my left hand up to the os uteri, and guided the tube into it as if I were using perchloride of iron; and in both cases the tube must have nearly touched the fundus of the uterus before I commenced to inject the hot water.

THE PRESIDENT.—I quite agree with Dr. Atthill that the injection of cold water is not always reliable, and sometimes does mischief. I have had no experience myself of the use of hot water in such cases.

DR. KIDD.—The injection of hot water into the uterus to check hæmorrhage comes before us in this country as something new—at least I presume from the silence of other members that they are in the same position as myself on the subject. Theoretically I can see that in many cases it is likely to be useful. I can quite understand that in the case of a woman in an exhausted state, with cold surface and nearly pulseless, hot applications would be more likely than cold to cause the uterus to contract; that water at 110° thrown into the uterus would prove a powerful stimulus, and act beneficially, as it appears to have done here. The suggestion has special importance for the reason mentioned by Dr. Atthill, that one can always have hot water at once. In many cases at the Coombe Hospital, when the patient is cold and exhausted, it has been the practice to employ hot and cold applications alternately. That prac-

tice I have adopted myself. We apply hot and cold napkins to the sacrum alternately, the object being to make a greater impression and restore sensibility. I am sure we shall all be very glad on many occasions to try the suggestion.

DR. M'CLINTOCK.—My experience only goes as far as Dr. Kidd's. I am sure we are all ready to admit that though cold is a most powerful means of checking hæmorrhage, yet like every other remedy it has its limits of utility, and that when used without discrimination it may do great harm. Baudeloque, a most acute and accurate observer, in speaking of the treatment for *post partum* hæmorrhage, mentions first the external application of warmth, and then goes on to say that cold in the form of either aqueous or spirituous fluid may be applied either to the uterus or the sacrum. I have ventured occasionally to follow Baudeloque's suggestion, and to apply warmth to the sacrum and over the pubis. I never ventured further. I have seen in some American journal a recommendation in favour of the injection of warm water for *post partum* hæmorrhage, but I felt timid about adopting it. I am encouraged by the experience and recommendation of Dr. Atthill to try it. I would not, however, have recourse to warm water in the first instance, but would rather use cold for a little while, and then the warmth would have an additional power of giving a shock to the uterus after the stimulating power of the cold had been exhausted.

DR. ATTHILL.—I have very little to say in reply. The subject is new to myself. It is about a fortnight since I first tried this treatment. In cases of sudden bleeding, where the vital powers are vigorous, cold water is a most efficient remedy, and in such a case I should not think of using hot water. I would say that hot water would be suitable in cases which would demand the injection of the perchloride. In the cases which I have mentioned I would certainly have used perchloride of iron had I not determined to try the effect of hot water; but if hot water proves after further trials to be an efficient remedy, I should prefer it for many reasons to the perchloride—amongst others, on account of the comfort it gives to the patient, and the extreme facility of its use. Cold water is admirable where you are able to give a shock; but you cannot give a shock where the patient is already depressed by cold, or exhausted by a long-continued drawing away of blood. Hot water would be peculiarly suitable in that form of secondary hæmorrhage which comes on an hour or two after delivery, in which the uterus is sometimes full of clots and the patient exhausted, and consequently unable to bear cold water. In such cases it will, I am satisfied, prove extremely useful. On the other hand, the indiscriminate use of hot water would be very injudicious; but the injection of hot water into the vagina alone would, I believe, be useless.

TRANSACTIONS OF THE CORK MEDICO-CHIRURGICAL SOCIETY.

SESSION 1876-7.

President—DR. STEPHEN O'SULLIVAN.

Secretary—DR. RINGROSE ATKINS.

Dermoid Cystic Tumour Removed from the Verge of the Anus. By Dr. OLIVER, Army Medical Department; and exhibited by S. O'SULLIVAN, M.D., and Ch.M., Q.U.I.; Surgeon to St. Patrick's Hospital, Cork; &c.

GENTLEMEN,—The pathological specimen now before you was kindly forwarded to me by Dr. Oliver, when the patient from whom it was removed was sent into St. Patrick's Hospital for treatment. It is, as you may observe, a dermoid cystic tumour, the history of which is, I venture to observe, one of the most remarkable on record.

Dr. Oliver gives the following short description of the case. He says :—“As it may be of some interest to you to know something of the early history of the case, I beg to say that I received her over from Dr. Nunan, Kinsale—who asked me to look after her during his illness—last Friday (9th March, 1877). Her case seems peculiar, and the growth, which at first appeared to be malignant in its various features, would now seem as if partaking of the nature of abdominal growths sometimes found connected with ovarian disease. It is difficult to understand how it has found its way into the rectum. The child states positively she has never interfered with herself in any way, and never suffered from any intestinal trouble or inconvenience until the Sunday previous, when she complained of a pain in the anal region, and on examination a small substance like a blindboil was found protruding. This rapidly assumed the dimensions of a tumour $1\frac{1}{2}$ inches by 2 inches in size; and its presence was accompanied on the following day, and since, by hæmorrhage, much fœtor, and severe burning and shooting pains in the region of the rectum, and left groin and thigh. There was no unusual swelling of the abdomen, and the ladies^a in attendance on her say they have much faith in the child's statement, and believe she has not tampered with herself in any way. The external tumour was removed by me this day (13th March, 1877) by ligature, and I forward it to you for inspection and further guidance.”

^a Sisters in charge of the Industrial School, Kinsale.

The child from whom this tumour was removed was admitted to St. Patrick's Hospital on the 14th of March, 1877, the day after the operation was performed. Her age is thirteen years. She presents a cachectic appearance, but is otherwise in good health, and free from pain.

Upon making an examination I found a small granulating surface, about one-fourth of an inch in diameter, immediately outside the verge of the anus, the situation of the pedicle of this tumour. The rectum was impacted with fæces, which were readily removed by enemata, and a purgative administered by the mouth. After a short stay in the hospital under a nutritive diet, she returned to her school in good health (3rd April, 1877).

My friend, Dr. Ringrose Atkins, to whom I am indebted for having made some microscopic sections of the tumour, gives the following description of its appearance:—"The tumour presented an ovoid appearance; was smooth, and of an olive colour, with a somewhat cutaneous looking surface, minute hairs growing from it in places. From its inferior lateral surface two masses of teeth projected, one consisting of a single well-developed molar, while the other was composed of three rudimentary teeth growing from the same root, one of these being covered by a prolongation of the surface of the tumour. On making a section through the tumour it was found to contain an irregularly shaped mass of bone, the soft part encapsulating this. The teeth just mentioned were embedded in this mass of bone, and on the surface exposed by the section was a pit, apparently for another tooth. A section of the enveloping capsule under the microscope showed it to consist of thickened connective tissue, and large masses of adipose globules, between which ran numerous vessels. The tissue composing the surface was dense and fibroid in appearance, and from it projected little free prolongations strongly resembling papillæ. The entire section presented the appearances of the deeper layers of the skin below the dermoid layer."

The rapidity with which this tumour descended from its original situation in the pelvis, and appeared externally, is remarkable. The question arises, was this tumour originally developed in the ovary, or was it one of those cysts which are occasionally congenital about the sacrum, and which are called tumores coccygei? (Billroth.)

I believe it is now fully established that the ovary is not the only organ in which these bodies are found. They have been removed from the testicle, the lung, and the anterior mediastinum, as well as from the jaws—these last are, according to Mr. Paget, "enlarged tooth capsules"—and from the sacrum. My friend Dr. Shinkwin, Surgeon to the Cork North Infirmary, recently removed from the upper lid of a young woman, a patient of his at the infirmary, a dermoid cystic tumour containing hair.

Professor Carl Schroeder says^a :—"Dermoid cysts very often remain stationary, and are then undiscovered during life." I have myself removed, *post mortem*, an ovarian dermoid cyst from a young woman who died of phthisis pulmonalis at St. Patrick's Hospital. He adds—"In many instances, however, they may rapidly undergo further development from some precise period, so that they grow rapidly, and behave clinically just like cystomata. Sometimes they discharge themselves into neighbouring organs, most frequently into the rectum or the bladder. . . . They may also break through the abdominal wall, and occasionally through the vagina. Perforation into the abdominal cavity is fortunately rare."

There are two theories advanced to account for the formation of these cystic growths—namely, the theory of Geoffrey Saint Hilaire, and that of Lebert. The first is called "fœtal inclusion;" "that is to say, it is conceived that the more or less imperfect germs of a second fœtus have become included in the testicle" or ovary "during development." The theory of Lebert, "heterotopie plastique," or "the production of these structures by forces existing in the part." The latter theory is considered by Dr. Humphry,^b "more in accordance with other teratological and pathological phenomena than the 'inclusion theory' of Saint Hilaire."

But, although one or other of these theories may be correct in most cases, I think the present case, as well as many others which have been recorded, will go far to prove the observation of Dr. West, that "these cutaneous cysts are found in circumstances which do not seem to admit of either of these solutions." Rindfleisch, in his *Pathological Histology*, Vol. II., p. 185, says :—"It may be that the peculiar nature of the ovary and testis, as generative glands, predisposes them to the production of dermoid cysts; but the notion that these cysts ought, therefore, to be regarded as rudiments of fœtal development is quite gratuitous, for dermoid cysts are also met with in other organs."

His^c believes "that the first rudiment of the genital organs is developed from the axial cord of His, in the formation of which the upper germinal layer also participates; and that the horny layer contributes chiefly to its formation. From this we can understand how formations of the external skin can originate from parts of the upper germinal layer which have not contributed to the formation of the ovary, and how fat, bones, teeth, &c., can be produced from parts of the middle germinal layer, which also participated in the folding in of the axial cord. Accordingly, the first trace or rudiment of dermoid cysts is always congenital.

^a *Cyclopædia of the Practice of Medicine*. Ed. by Dr. H. Von Ziemssen. Vol. X., p. 435.

^b *Holmes' System of Surgery*. Art., Diseases of the Male Organs of Generation. P. 599.

^c *Cyclop. of Pract. Med.* Ed. by Ziemssen. Vol. X., pp. 432-3.

Their further development, as a rule, begins after puberty, but in exceptional cases even earlier."

It is probable that this dermoid cystic tumour originated congenitally in some part of the pelvic cavity—possibly the ovary; that as it grew it descended gradually in the intercellular space, and finally pointed as described in the anal region.

SESSION 1877-78.

President—DR. M. O'KEEFFE.

Secretary—DR. RINGROSE ATKINS.

Opening Address. By the President, DR. M. O'KEEFFE, Professor of Therapeutics, Queen's College, Cork.

GENTLEMEN,—It is with a feeling of regret and somewhat of reluctance, I find myself in the position in which your kindness has done me the honour to place me this evening. I regret that some gentleman more able was not selected to fill the place of President of your Society; and I feel reluctance at undertaking the performance of duties, which I find myself unable to discharge in any way commensurately with the ability with which they had been discharged in former years.

In addressing this Society, I feel a peculiar difficulty, as I am conscious that I am speaking in the presence of my seniors and superiors in the profession. In the presence of the men from whom I have had my earliest and most impressive lessons, in the various subjects of our common profession, and whose teaching has, in a great degree, shaped and guided my professional views and conduct, for the years I have been engaged in practice, and whose valued friendship has been a stay, a guide, and a protection to me on many a doubtful and dangerous occasion.

In assuming the presidency of this Society for the ensuing session, I feel I am undertaking a very grave responsibility; however, having undertaken it, I must try and discharge its duties to the best of my power.

It would be out of place in me to occupy, or more correctly to waste, your time in any attempt at an inaugural address, nor does the present occasion call for such. We have those addresses at present in the weekly periodicals—usque ad nauseam—we are met here to enter on a new session, and I trust it will be, like the past session, characterised by honest and honourable work.

We cannot enter more profitably on the work of the coming session than by reviewing the proceedings of the Society for the past year.

Anyone looking over the minutes of the meetings, as taken down by our gifted, valued, and indefatigable secretary, must be struck by the

amount of work done, as well as by the character of that work in the fourteen meetings we had during the past session; and must appreciate the immense advantages those meetings must necessarily have conferred on the members of the Society, when most important questions connected with every professional department were illustrated, some by pathological specimens, often by reports, or the exhibition of apparatus, as the case required. Now the examination of a pathological specimen, which, in all cases, is useful and instructive to every practitioner, whether he makes pathology a subject of special study or not, becomes much more valuable when studied in connexion with the history of the individual during life, reported by an observer of whose competence, honesty, and ability we are certain from personal knowledge, and on the accuracy of whose observations we know perfect reliance can be placed.

One such specimen, studied under conditions so advantageous, is calculated to give an infinitely better idea of the nature of the disease than the most polished discourses, or carefully written treatises; but, when such an examination is accompanied, or immediately followed by a discussion which elicits the opinions of men of vast experience, extensive reading, and profound learning, refined, and no doubt sometimes corrected by many years of vast clinical experience, I am justified in describing the advantages conferred by this Society on its members as immense.

This of course refers more especially to us the junior members. I would not, however, altogether except our most respected seniors, to whom we owe a deep debt of gratitude for coming here night after night, often at considerable personal inconvenience, influenced mainly by a sense of what is due to the profession and the public, encouraging and stimulating their less experienced professional brothers; but even to them the expression of Sallust in contrasting the powers of the mind with those of the body may fitly apply—*Alterum alterius auxilio eget*—One requires the aid of the other; or as another commentator has it, and which more aptly applies in this case—*Alterum alterius auxilio veget*—One grows strong by the aid of the other.

So far with respect to our pathological specimens. We are not, however, merely a Pathological Society, we receive reports of cases, generally those occurring in hospital practice, though there is no objection to reports of cases in private; on the contrary, we are anxious to receive such reports, and when we take into account the population of Cork, and the thousands of cases that must annually occur of immense interest in a medical, surgical, and gynæcological point of view, we must, I think, necessarily conclude that the body of the profession, especially its junior members, owe little gratitude to their more favoured brothers, who, from apathy, indifference, or laziness keep these mines of knowledge all to themselves locked up in their own memories, instead of generously coming forward here, and laying before the Society the results of their labours

and experience, results undoubtedly gained by years of careful and faithful scientific observation, and therefore all the more valuable to a society like ours, which necessarily contains many junior members. In addition, our evenings were very frequently, indeed pretty generally, made delightful by the exhibition of the most perfect microscopic preparations of the morbid specimens exhibited. This I consider one of the most attractive features of our meetings.

Nothing could exceed the beauty, delicacy, and truth of these preparations as faithful representations of the results of diseased processes. We have thus had the advantage, I may say the privilege, of having placed clearly and vividly before us the effects of inflammatory and other diseased actions on the various organs of the body—brain, muscle, gland, bone, &c.

To the Secretary, Dr. Ringrose Atkins, I personally feel deeply indebted, and I am certain every member of the society joins heartily with me in expressing our sense of the great services he has rendered the society in not only being the very model of a loyal and industrious secretary, but also in placing at its disposal, in the most generous and unselfish manner, the results of his, I may almost say, unrivalled microscopical skill and knowledge.

Another feature in the work of the session was the exhibition of surgical and scientific instruments and apparatus. I shall allude to this again in giving an analysis of the year's work.

Before leaving this part of the subject I may be permitted to make an allusion to the nature and manner of our discussions during the past session. These rendered attendance at a meeting a source of real pleasure. There was, of course, difference of opinion to some extent, more or less, on every subject started. Were not this the case, the discussion would indeed be stupid and useless—in fact, would degenerate into a series of tame and useless lectures; but, while each member supported his opinions with such arguments or facts as came within the range of his experience, there was no heat—everything was spoken in the most temperate manner. If anyone still considered his original opinion correct, he was, of course, free to maintain it. We had, for the session, no instance of the “I am Sir Oracle, and when I ope my mouth let no dog bark.” Hence, our meetings were all most harmonious—and why? because there was not a member attending who spoke for the purpose of listening to his own eloquence, and who would persist in ringing the changes on a set number of words as often as the theory of combinations would allow. Our members expressed what they had to say on any given subject in the fewest possible number of words. Thus an opportunity was given to every gentleman to express his opinion. We were not bored with long speeches and frequent reiteration; neither did we find ourselves lectured to as if we were a number of schoolboys or mere tyros.

I trust I am not trespassing too much on your patience, but instances came under my observation, in former years, where gentlemen expressed to me their intention of withdrawing from this Society rather than having to be bored with listening to lectures derived from Kirke's physiology or other students' vade mecums.

I shall now review, as briefly as possible, the work done by the Society during the past session, taking the cases in the order in which they were brought before the Society.

The session opened with a most interesting specimen of nodular carcinoma of the liver, occurring in a woman whose breast had been amputated a month previously for scirrhus, which also implicated the axillary glands. The amputation, carried out under Lister's antiseptic method, was as successful as a surgical operation could be; but this case, where we have an exceedingly rapid growth of cancer in a visceral organ after the removal from the breast, goes far to illustrate the inadvisability of removing scirrhus of the breast, except under the most favourable circumstances, more especially when the axillary glands are engaged.

On the same evening a most important specimen was exhibited by Dr. Ed. R. Townsend, which throws considerable light on that melancholy class of cases which an accoucher meets with occasionally—I mean the sudden and totally unexpected death of a woman shortly after her confinement. This specimen was a heart containing a polypus, or thrombus, more or less organised, and adherent to the walls, extending into the auricle and probably into the pulmonary artery. The woman, who had a very deformed pelvis, was delivered with great difficulty, ovariectomy having been had recourse to after a fair trial with the long forceps. Under these circumstances the uterus could not escape injury. The case shows how a clot may form in a uterine vein or sinus, pass into the circulation, be arrested or entangled in the chordæ tendinæ of the ventricle, where it is certain to increase in size by deposits from the blood passing over it. Now, we know that the fibrin of the blood may coagulate in the vessels, under various conditions, and this interesting case opens up for consideration the question—may not sudden death, unconnected with parturition, of an individual who never complained of any disease, be produced in a similar manner?

At the second meeting Dr. O'Connor exhibited the lungs and heart of a man who was for a long time the subject of pulmonary consumption. There was nothing unusual in the appearance of these organs. The lungs were studded with cavities and tubercles, and the heart was atrophied, pale, and fatty; but the mode of death was altogether different from what we find in the consumptive. The man one day uttered a loud cry as if in intense pain, fell down convulsed, and immediately expired. The kidneys were intensely congested, and there was a considerable amount of serum in the pericardium. This case led to a most instructive discus-

sion; Drs. O'Connor, Finn, &c., thought death was due to paralysis of the heart, from pressure of the pericardial fluid. Dr. William Townsend, in his usual able manner, attributed death to sudden suppression of kidney functions. In this he was supported by Drs. Shinkwin, E. R. Townsend, and others. Dr. Atkins held the cause of death to be cerebral anæmia, dependent on weakened or paralytic heart. On the whole the majority of the members present held views similar to those expressed by Dr. Townsend.

On the same evening Dr. D. C. O'Connor, jun., reported a case of ruptured uterus in a woman who had previously borne thirteen children. The symptoms and history were the usual ones. The case elicited a discussion as to whether the forceps should be used in the tedious labour of a multipara. Dr. W. C. Townsend thought the practice might be useful. Dr. Finn, on the other hand, was of opinion that as in these cases the rupture depended on degenerative changes in the uterine tissue, the rupture would occur all the same, and the forceps and its owner get the credit of it—a view in which the majority of the members present coincided.

At the third general meeting, Dr. Corby exhibited a penny-piece which had lodged for five days in the œsophagus of a boy of thirteen. The doctor succeeded in getting a probang below the coin, and thus skilfully extracting it.

At this meeting Dr. Shinkwin brought forward a case of accident requiring amputation below the elbow. The notes of the case offered an opportunity for a useful discussion as to the relative merits of primary and secondary amputation.

At the fourth general meeting the secretary, Dr. Atkins, exhibited the heart and great vessels of an inmate of the asylum who died suddenly—in fact, as Dr. Atkins thinks in this case, passed gently into death. The aorta was dilated into a fusiform and dissecting aneurism. The pericardium contained a large clot, and the liver was strongly hobnailed. The man was an inmate in the asylum for thirteen years. There was no history of previous intemperance; he never complained of any disease. The case elicited a lengthy and most instructive discussion. We have serious pathological changes in two of the most important organs of the body, leading in the end to a very sudden death—yet during life there was no symptom to give a timely warning.

At this meeting Dr. Corby brought forward the case of a young girl who had convulsive attacks after an accident. After having discharged her stomach the convulsions ceased. In the vomited matter Dr. Corby discovered portions of a lumbricoid worm, to whose presence he attributed the convulsions, and called attention to the medico-legal bearings of the case. This produced a discussion on the influence of worms in causing various reflex nervous actions.

At the fifth meeting Dr. D. C. O'Connor, jun., exhibited a tumour which had been removed at the Mercy Hospital from the left lumbar region of a lady, partly by the galvanic ecraseur, partly by Paquelin's benzol vapour cautery. It resembled a gigantic nævus, bled frequently, and grew repeatedly. Dr. Atkins exhibited beautifully prepared microscopic preparations of the tumour. The huge pyramidal polynucleated cells, with the delicate stroma, left no doubt of the correctness of the diagnosis—*i.e.*, malignant tumour. Though the neighbouring parts were extensively cauterised, the tumour was reproduced with amazing rapidity. The sufferings produced by the secondary growth were much greater than those of the primary, and caused the staff of the hospital to regret having interfered. For myself, I must say, that this case tended much to strengthen a conviction that has been for some time growing in my mind—*i.e.*, that surgical interference increases the sufferings and shortens the life of a cancer patient, at least, in ninety-nine cases out of a hundred.

On this evening Dr. Atkins reported a case of suicidal hanging, where the body was cut down before life was extinct, and respiration restored by artificial inspiration and faradisation. She died, however, of congestion of the brain, thirteen hours afterwards, and never became conscious. Venesection was had recourse to with temporary improvement. This led to a discussion as to the propriety of having recourse to venesection in congestions generally, but more particularly those intracranial. Dr. Cremen instanced a case where life had been restored after partial hanging by drawing blood from the jugular vein. The general sense of the meeting was in approval of the practice in such cases.

At the sixth general meeting Dr. Cremen brought forward the heart and kidneys of a woman who was admitted in a comatose and dying condition. Heart weighed twenty-one ounces, its openings were all enlarged, especially the right auriculo-ventricular. The body and organs generally, especially the brain, were in an œdematous and sodden condition. The kidneys, however, were small and hard. Some exquisite microscopic preparations from heart and kidney were subsequently exhibited by Dr. Atkins. They showed the kidney lobules, and malpighian bodies surrounded by thick zones of dense fibrous tissue, while the hypertrophic heart was seen to be undergoing a process of fatty degeneration. The exhibition of the specimen led to a most important discussion on the relation of cardiac hypertrophy to renal diseases.

Subsequently, on this evening, Dr. O'Keeffe brought before the notice of the Society a case of splenic enlargement occurring in a soldier's wife who never had suffered from ague. The swelling was first noticed after her confinement, and was diagnosed ovarian; however, when he saw it, there was no doubt it was splenic; it was exceedingly hard and knobby. The only question was whether it was fibroid or malignant. She presented an anæmic appearance, and her blood contained a large excess of

white corpuscles. She had all the accompaniments of defective nutrition. After leaving Mercy Hospital I next heard of her as an inmate of St. Mary's, London, where her case was diagnosed as malignant disease of the spleen.

The Society was engaged at its seventh meeting with an exhibition of instruments, exhibited by Messrs. Lester and Harrington. The meeting proved to be a most interesting one; and the feeling of the Society was that such exhibitions should be encouraged in future sessions.

At the eighth general meeting Dr. Cremen reported a curious case of right hemiplegia, and right hemi-anæsthesia occurring in an old woman, who was found unconscious; under treatment she slowly recovered consciousness, but soon died suddenly. The *post mortem* revealed meningeal hæmorrhage over second hemisphere. The case was peculiar, as the paralysis occurred at the same side as the cerebral lesion.

Subsequently Dr. Cremen reported a rare and interesting obstetric case. He suspected a twin gestation; in the course of the labour the woman got a syncopal attack and immediately expired. The *post mortem* discovered twins, one placenta detached, and uterus filled with clots. A discussion on the appropriate treatment in such a case occupied the meeting for the remainder of its sitting.

The ninth general meeting of the Society was rather a sad one. The meeting was postponed, and no business transacted in consequence of the lamented death of Dr. Thomas Gregg, an old member and ex-president. All I can say is that any compliment paid by the Society to the memory of that distinguished surgeon and physician was really merited; taking him in his professional capacity or as a man and a friend he was one whose equal it is no easy matter to find.

At the tenth meeting the President, Dr. O'Sullivan, exhibited a rare and unique specimen of a dermoid cyst, removed from the side of the anus of a girl of thirteen. It contained a bony mass resembling an alveolus, from which projected a number of teeth about the size of what may be expected to be found in the jaw of a child of thirteen.

An interesting discussion of the nature of those cysts followed, when Dr. O'Connor reported a case of right hemiplegia which terminated in recovery. The report gave rise to a useful discussion on the nature of nervous diseases in general.

At the eleventh general meeting a tumour, fibro-cellular, was exhibited by Dr. O'Keeffe; after which a neat and simple apparatus was exhibited and explained by Mr. C. O'Keeffe, county and city analyst, for the quantitative estimation of urea, devised by Mr. O'Keeffe and Professor Maxwell Simpson.

At the twelfth meeting Dr. O'Keeffe exhibited a pathological specimen of scrofulous knee-joint, with extensive disease of the bones on each side of the joint—the pulpy degeneration of Sir B. Brodie. Amputation

above the joint was performed; the case turned out well; cicatrisation was complete within fourteen days after the operation; there was no suppuration. He took the opportunity of calling the attention of the Society to the well-known antiseptic properties of chloral hydrate. In this case Lister's method was dispensed with; the stump kept covered with a solution of chloral hydrate, and the dressings never removed.

I fear I have wearied you with this detail, but I could not otherwise show the amount and importance of the work done by the Society. Two other meetings were held, but owing to unavoidable causes the minutes were not entered in the minute-book. The work done at those twelve meetings, and the opinions elicited from the experienced gentlemen who took part in the discussions, or brought forward the specimens and cases, justifies me in my former statement that its members, senior as well as junior, derive immense advantage from this Society, and should for their own sakes, if for no other, do all in their power to promote its interests and increase its usefulness. The *résumé* of the work done also shows that the past has been a very successful session, and that we have at hand sufficient material, if properly worked up, to render our meetings not only highly interesting, but also useful and instructive. But now about the future. Consider the number of noteworthy cases in hospital and private practice that present themselves daily and are never reported. Many of us, especially the younger members of the Society, are under the impression that a case is not worth being brought forward unless there is something very striking about it—something very much out of the common. This is a great mistake. There is scarcely a case to be met with in hospital, dispensary, or private practice that has not a certain individuality in itself, the consideration and discussion of which would develop much interesting knowledge and research. There is not a member in this Society, from its Nestor to the latest-joined junior, who could not, if he would, furnish many cases suitable for profitable discussion.

Let us stir ourselves, then, and try to make the coming session as fertile of results as the past, or more so if possible. Let every member try to bring at least one case or one specimen, and we shall have enough. To our younger members I may say the Council of the Society will receive their contributions with especial favour and regard.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

*Of Eight Large Towns in Ireland, for Four Weeks ending Saturday,
December 29, 1877.*

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	
Dublin, -	314,666	649	750	9	45	—	7	12	22	11	31·0
Belfast, -	182,082	475	365	—	9	7	1	2	26	4	26·0
Cork, -	91,965	162	215	—	28	—	—	—	6	5	30·5
Limerick, -	44,209	79	82	—	2	2	—	—	—	2	24·3
Derry, -	30,884	54	48	—	—	2	—	—	—	—	20·3
Waterford, -	30,626	45	63	—	7	—	—	1	3	1	24·5
Galway, -	19,692	41	22	—	—	—	—	1	1	3	14·5
Sligo, -	17,285	27	24	—	—	—	—	2	1	1	18·0

Remarks.

A very high death-rate prevailed in Dublin and Cork; in Belfast the mortality was high, but in the other towns it was moderate or low for the time of year. In London the death-rate was 24·7 per 1,000 of the population annually, in Edinburgh 26·8, and in Glasgow 24·7. Even omitting the deaths of persons admitted into public institutions from localities outside the registration district, the death-rate in Dublin considerably exceeded that of the other cities. It was 29·9 per 1,000. The deaths from zymotics were 137, compared with 129, 135, 110, 91, and 105 in the preceding periods. The average number in the corresponding period of the previous ten years was 127·4. Scarlatina did not cause a single death, at least directly, but measles continued to be very destructive to life. Fever showed an increased fatality, and small-pox caused 9 deaths, compared with *one* in the preceding four weeks. Of the 22 deaths from fever, 6 were due to typhus, 10 to typhoid, and 6 to simple continued fever (?). In Belfast fever was very fatal, and the

epidemic of measles still raged in Cork. This zymotic caused 7 deaths in Waterford. The mortality from respiratory affections was again very high in Dublin. They caused 195 deaths, the average number in the same period of the previous ten years being 166·7. Bronchitis caused 145 deaths (average = 132·7), and pneumonia 33 deaths (average = 18·6). Small-pox killed 116 persons in London, compared with 97 and 45 in the two preceding periods. Measles has also become epidemic in London, where 379 deaths from this disease were registered in the four weeks.

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of December, 1877.

Mean Height of Barometer,	-	-	-	30·004 inches.
Maximal Height of Barometer (on 19th at 9 p.m.),	-	-	-	30·700 „
Minimal Height of Barometer (on 6th at 9 a.m.),	-	-	-	29·159 „
Mean Dry-bulb Temperature,	-	-	-	42·0°
Mean Wet-bulb Temperature,	-	-	-	40·6°
Mean Dew-point Temperature,	-	-	-	38·8°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·236 inch.
Mean Humidity,	-	-	-	88·5 per cent.
Highest Temperature in Shade (on 12th),	-	-	-	53·7°
Lowest Temperature in Shade (on 28th),	-	-	-	30·2°
Lowest Temperature on Grass (Radiation) (on 28th)	-	-	-	24·9°
Mean Amount of Cloud,	-	-	-	62·9 per cent.
Rainfall (on 17 days),	-	-	-	2·330 inches.
General Direction of Wind,	-	-	-	W. and S.W.

Remarks.

December was generally an open month, with very little severe frost, although hoar-frosts were of frequent occurrence. A very rapid increase of atmospherical pressure took place on the 1st and 2nd, when a polar air-current set in, with a cold, foggy, damp atmosphere. A large and deep depression travelled across the United Kingdom on the 6th. After sunset of the 8th the crescent moon and Venus shone brilliantly in the south-western sky. Broken weather succeeded, due to the passage north-eastwards across Ireland of three distinct areas of atmospherical depression. During this period temperature was above the average for the season. An anticyclone formed in the S.W. and S. on the 14th. This area of high atmospherical pressure, with its accompanying slight barometrical gradients, persisted until the 21st. The anticyclone reached its fullest development on the 19th, at 9 p.m. of which day the barometer stood at the exceptional height of 30·700 inches in Dublin. Hoar-frosts occurred at night about this time. From the 22nd several cyclonic

systems travelled eastwards across Scotland, and temperature became much lower, with showers of hail, sleet, and snow on the 25th and two following days. On the night of the 27th the thermometer in Dublin fell to $30\cdot2^{\circ}$ in the shade, and to $24\cdot9^{\circ}$ on the grass. Next morning broke with an overcast, lowering sky, and at 9 a.m. hail and snow began to fall. In a few minutes rain succeeded, and an extraordinary rise of temperature took place as a new depression advanced from S.W. The thermometer at 9 p.m. read $51\cdot2^{\circ}$, or 21° higher than 12 hours previously. The rainfall of the month was about the average. Snow, sleet, or hail fell on the 12th, 25th, 26th, 27th, and 28th. High winds prevailed on the 5th, 6th, 7th, 9th, 11th, 12th, 15th, 22nd, 24th, and 26th.

RAINFALL IN 1877,

At 40, Fitzwilliam-square, West, Dublin.

Month	Total Depth	Greatest Fall in 24 Hours		Number of Days on which $\cdot 01$ or more fell
	Inches	Depth	Date	
January, - - -	4·322	·950	3rd	25
February, - - -	1·560	·421	13th	19
March, - - -	2·741	1·150	24th	20
April, - - -	4·707	1·095	15th	21
May, - - -	2·343	·440	16th	18
June, - - -	·921	·237	6th	12
July, - - -	3·300	·770	15th	25
August, - - -	3·536	·625	18th	24
September, - - -	1·795	·650	14th	10
October, - - -	2·153	·639	15th	16
November, - - -	2·438	·569	21st	22
December, - - -	2·330	·570	28th	17
Total, - - -	32·146	—	—	229

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

ADMINISTRATION OF QUININE.

SPEAKING of the treatment of malarial fever, Dr. Thomson, Professor of Materia Medica and Therapeutics in the University of New York (*N. Y. Med. Record*, Nov. 17, 1877), makes the following observations:—"The administration of the quinine in this case—*i.e.*, fever and ague—should be preceded by a cathartic. It will facilitate its absorption. What is it that embarrasses absorption of quinine in these cases? It is gastric, supervening upon portal, congestion, thus hindering the absorption. Quinine, morphine, and all the vegetable alkaloids, if they remain for much length of time in the alimentary canal, and are subjected to the action of the ordinary chemical fluids there, become so changed as to lose largely their special properties. These remedies, therefore, should be given upon an empty stomach. It is not unfrequently the case, as is well known, that quinine, on account of disturbance in the stomach, is not absorbed, even if it be retained. In such cases resort may be had to the hypodermic use of the drug." "Is there any means," Dr. Thomson asks, "by which the effective dose of quinine can be diminished? Capsicum combined with quinine will diminish the size of the dose requisite, and the same may be said of ginger and other aromatics. A good dose of capsicum combined with twenty grains of quinine will act as well as thirty grains of quinine without the capsicum. Spices in general stimulate the portal circulation, and promote the flow of bile, and hence their universal use in hot climates. There is a tendency on the part of quinine and capsicum to purge, and sometimes to purge violently. In such cases the purgative action is caused by the increased flow of bile produced by the capsicum. Ginger and quinine, when combined, do not purge, and it makes a very good combination. If the medicine is administered in form of pills, capsicum may be preferable, because of the less bulk required; but, if desirable, the ginger may be given separately, and with the same effect as when combined with the quinine. The proportions should be one grain of capsicum to three of quinine; with ginger, one grain of each. There is constant failure in the treatment of malarial poisoning by the use of quinine, and nearly always it arises from the manner in which the remedy is administered. The point to be obtained is the quick absorption of the quinine. Suppose, for example, you are called upon to prescribe in a case of malarial poisoning in which there is almost continuous vomiting, as in bilious fever. If

there is gastritis present, there will be tenderness upon pressure at the pit of the stomach and in the region of the gall-bladder; there is apt to be some swelling of the epigastrium, and the patient vomits as soon as anything is taken. It is useless to administer quinine by the mouth under such circumstances, because the excessive irritation which it produces upon an inflamed mucous membrane causes its rejection at once. If injected into the rectum under the same circumstances, it will not succeed any better, because rectal absorption is diminished on account of portal obstruction. Now, if you will apply two or three leeches at the epigastrium, the vomiting will be arrested almost certainly, and you will be able to get the quinine absorbed. Do not use either mustard or blisters here to arrest the vomiting, for they are vascular stimulants. Topical blood-letting, on the other hand, is a prompt vascular sedative."

OPHTHALMIA IN NEW-BORN INFANTS.

DR. LUTON, of Reims, advocates the use of iodine dissolved in cherry-laurel water. Ten drops of the tincture in ten grammes of cherry-laurel water make a colourless mixture, leaving no precipitate, while in the same quantity of distilled water a coloured precipitate would soon form. The decoloration is owing to the production of hydriodic acid and iodide of cyanogen, two colourless bodies in solution. A mixture containing one part of tincture of iodine to twenty parts of cherry-laurel water is a collyrium of incontestable power in the purulent ophthalmia of infants. The liquid is to be dropped between the lids five or six times a day, besides external applications. In efficacy it is declared to be superior to nitrate of silver, while it has the advantages of being both painless and safe.—*Revue Médicale*.

S. W.

TREATMENT OF PITYRIASIS CAPITIS.

M. VIDAL, believing that pityriasis arises from the predominance of the sudoriparous glandular system over the sebaceous, thinks the first condition of cure lies in supplementing the deficiency of the latter. With this object he employs pomades composed, not of animal substances, which often prove irritating to the skin, but of vegetable fats. He prescribes usually the following formula with the best results:—

Cacao butter	-	-	-	-	1 ounce.
Castor-oil	-	-	-	-	5 ounces.

These form a pomade which can be perfumed according to taste.—*Paris Médical and Revue Médicale*.

S. W.

THE TREATMENT OF ECZEMA IN CHILDREN.

MR. A. ALLBUTT writes in the *British Medical Journal* of November 24: If there be any of the characteristic discharge usual in cases of acute inflammatory eczema in the ichorous stage, I advise, as a soothing application, the following remedy: Olei amygdalæ dulcis ʒij; olei amygdalæ amaræ ℥xx: mix; to be painted over the affected surface two or three times a day. If the disease be in the squamous stage, pure terebene is a certain application, to be used in the same manner. As regards internal treatment, I suggest dilute nitric acid in gradually increasing doses, according to age, combined with decoctum cinchonæ flavæ, and perhaps the administration of cod-liver oil. A diuretic might be of service. Sometimes the Vals water, or, if there be any anæmia, the Pyrmont or Tunbridge Wells water, is of use. Of course, many circumstances have to be taken into consideration in the treatment of eczema as to causes, complications, diathesis, etc. The above should be adopted, with nutritious diet, good air, and avoidance of stimulants.—*Lond. Med. Record.*

A FOREIGN BODY IN THE ŒSOPHAGUS, WITH PERFORATION OF THE AORTA.

DR. ASCHENBORN relates the following case in the *Berliner Klinische Wochenschrift* for December 10th:—V., a joiner's apprentice, was admitted to the Bethany Hospital on July 1st, 1876. That morning he felt severe pains during respiration, but had two days before begun to suffer from pains in the epigastric region, and along the gullet after swallowing a hard morsel of bread. On admission, there were accelerated costal and shallow respiration, a full pulse of 100, a temperature of 3·90° Cent. (102·2° Fahr.). The heart and lungs were normal. There was acute sensibility to pressure at the pit of the stomach and about the insertion of the diaphragm, but no difficulty of swallowing. During the next six days the pulse rose at times to 120, with febrile symptoms, and deglutition became extremely painful, except in the case of fluids. The respirations rose to 40 per minute, and were throughout costal and shallow; the area of cardiac dulness was extended. The neck swelled in the supraclavicular spaces, especially on the right side. On the 5th a longitudinal incision was made in the posterior wall of the pharynx, yielding an offensive sanious fluid of a dark colour, but no pus. After this, his general condition improved somewhat, but the swelling of the neck was not diminished. On the 7th, in the forenoon, two bloody stools were suddenly passed, his strength and general condition remaining unchanged for that and the following day. During the night of the 8th-9th July a copious stool of pure blood was passed, followed quickly by collapse. Under the use of port wine and camphor, the patient rallied somewhat during the day; but on the same evening, without any warning, immense hæmor-

rhage occurred from the mouth, and he succumbed in a few minutes. The necropsy revealed a longitudinal rent of two-fifths of an inch in the œsophagus, about four inches above the cardia, at right angles to which was found a needle two inches long, piercing both walls of the descending aorta from before backwards. Both punctures were covered by small, firmly adherent blood-clots. The surrounding tissue, from the œsophageal to the posterior pharyngeal wall above, was infiltrated with sanguineous ichor. In the œsophagus were found large blood-clots, the stomach was completely filled with a large coagulum of blood, and the whole intestinal canal contained large masses of clotted blood. The needle, which was swallowed with the bread, became fixed in the œsophagus, and caused the moderate pain of the first few days. By repeated acts of swallowing it was made to penetrate the aorta; hence the bleeding on the 7th. The clots then formed served for a couple of days to plug the punctures; but when they became decomposed under the influence of the food taken, there resulted further and fatal hæmorrhage.—*Lond. Med. Record.*

QUININE IN GONORRŒA.

M. HABERKOM finds the following injection most useful in cases of long-standing gleet:—

Sulphate of quinine	-	-	-	6 grains.
Glycerine	-	-	-	2 drachms.
Dilute sulphuric acid	-	-	-	6 drops.
Distilled water	-	-	-	6 drachms.

A teaspoonful to be injected twice or thrice daily.—*L'Union Médicale.*

S. W.

FORMULÆ.

THE following formulæ are taken from a collection in the *Philadelphia Medical and Surgical Reporter* for December 15th:—*Dusting Powder for Chafing and Eczema of Vulva.*—℞. Calomel, ʒss.; calaminæ, ʒiss. M. Sig.—Apply several times a day. *Opium.*—The following is recommended as a form to give to those with whom it disagrees: ℞. Pulveris opii, gr. j.; potassæ carbonatis, gr. x. M. Dissolve in half a wineglassful of water. *Chilblains.*—The following is to be painted on with a camel's hair pencil two or three times a day. It gives sure relief, and if applied in the earliest stages it will speedily cure: ℞. Collodion, ʒvj.; tincturæ ferri chloridi, ʒij.; olei ricini, gtt. v. M. *Podophyllin Pill.*—℞. Podophyllin; ext. hyoscyami, āā gr. iij.; saponis, gr. iv. ss.; syrupi simplicis, gtt. vj. M. Divide in pilulas xii. *Gallic Acid.*—The *Canada Medical Record* gives the following formula for gallic acid: ℞. Acidi gallici, ʒj.; glycerinæ, ʒj.; aquæ bullientis, ʒv. M. A tablespoonful *pro re natâ.*—*Lond. Med. Record.*

In Memoriam.

WILLIAM STOKES,

M.D., D.C.L., F.R.S., &c.

Obiit Jan: 7, 1878, anno ætatis 74.

"All that lives must die,
Passing through Nature to Eternity."

It is but fitting that the pages of this Journal, so often enriched by the facile pen of the great Physician who has lately passed away, should pay some slight tribute to the memory of the illustrious dead. How best to discharge that pious duty has been a cause of much anxiety to the writer of this brief memoir of his teacher, benefactor, and friend.

It has been decided, after much consideration, to restrict the present memoir to an account of the life and writings of one of the ablest Physicians of modern times. Accordingly only incidental reference will be made to the social life and family history of Dr. Stokes—themes which will surely receive full justice, one day, at the hands of a more worthy biographer.

Should the following pages recall some memories of former days to Dr. Stokes' contemporaries or friends—afford encouragement to some weary, over-worked physician—and kindle a spark of enthusiasm, however feeble, in the breast of one of those who have adopted medicine as their profession—then this attempt to record the triumph of diligence, talent, intellect, large-heartedness, humanity, will not have been made in vain.

WILLIAM STOKES was born in Dublin in the year 1804. He was the second son of Dr. Whitley Stokes, who was elected a Fellow of Trinity College, Dublin, in 1787, and co-opted a Senior Fellow on Trinity Monday, June 10, 1805. Dr. Whitley Stokes resigned his Fellowship

on being appointed Lecturer in Natural History, June, 1816. He afterwards became Regius Professor of Physic in the University of Dublin, Nov. 13, 1830, which distinguished office he held until his death on April 13, 1845, when his illustrious son—the subject of this memoir—succeeded him.

The family of Stokes, which for several generations has occupied so prominent a place in Irish social, literary, and scientific circles, was of English origin—at least one representative of the family still resides near Nailsworth, in Gloucestershire. In *Gilbert's History of Dublin* mention is made of some works on engineering, written by the first of the family who was connected with that city—the Deputy Surveyor-General of Ireland for the time being. Two of this gentleman's sons became, in time, Scholars of Trinity College, Dublin—John Stokes in 1739, and Gabriel Stokes in 1751. Both were afterwards elected Fellows of the College, and both ultimately accepted College livings—Gabriel being also Chancellor of the Cathedral of Waterford. The Reverend Gabriel Stokes was father to Dr. Whitley Stokes, of whom we have already spoken.

Such was the ancestry of William Stokes. It may seem strange that one whose father, grandfather, and granduncle had all been fellows of Trinity College, Dublin, never became an undergraduate of the national University of Ireland. Yet so it was, and we only know that it was through no fault of his that Stokes failed to reap the great advantages which accrue from an university education in Arts. No one ever more fully realised the importance of such an education, and all his life through he maintained that to the medical man an intelligent and liberal University training was almost indispensable. In a "Prelection," introductory to the Medical Session of 1855-56, delivered before the University of Dublin in his capacity as Regius Professor of Physic, we find these sentences: "Gentlemen—You have adopted Medicine as your profession. Let me ask you, have you thought on the nature of the road you are about to travel; do you expect to move onwards as on a beaten path, like machines, and not like men? Do you look at your profession as a mere means of livelihood, to be reached by a course that can lead only to mediocrity, or will you strive to fit yourselves for the highest place, by a full moral and mental culture? I trust you will. . . . Reject

as false, and utterly mischievous, whatever you may be told as to the inutility of a broad and liberal extra-professional education; you are not going to a trade, but to become members of an honourable profession, and therefore it behoves you to consider whether you will sacrifice your chances of rank and success by following those bad advisers who tell you, for their own purposes, that an exclusive education will serve your purpose. I warn you against such doctrine. A few years will show you that it is contrary to the spirit of the age. Do not believe that, with your minds unopened, unrefined by literature, untrained to accurate reasoning—with your tastes undeveloped and your habits untutored—the mere fulfilment of your medical or surgical curriculum will qualify you to support the honour of your profession, or place you in the class of the workers and pioneers of a progressive science. . . . If the degree in Medicine were given without Arts, it would be at once degraded. Our Bachelors and Doctors of Medicine would be placed in an inferior rank to those of Oxford or Cambridge. The step would be nothing but a yielding to the system of exclusive education, so injurious to our real interests. Do entirely away with the Arts education for the physician, and you at once remove his claim to be placed on an equal footing with the Clergyman or the Barrister.”

But the education of the author of these weighty words was by no means neglected. Young Stokes learned under the supervision of excellent tutors; and, as his father's companion and assistant, “his mind,” (to quote a recent biographer in the *Dublin University Magazine*) “early turned to the original study of nature, developed those broad principles and that originality of thought which are the only true basis of success in any science or profession. In his consistent advocacy of an Arts education, as a necessary preparation for all those who are about to make medicine their study, he was actuated not alone by a zealous interest in the student's welfare, but by an ardent desire to elevate his profession and promote its honour—objects which he rightly judged can best be attained by recruiting its ranks only with men of education, capable of commanding the respect and confidence of their fellow-men.”

Having entered upon his twentieth year, William Stokes proceeded to Edinburgh, where he matriculated at the University as a student of medicine. He at once became a “practising pupil” in the fullest sense—

a student of disease at the bedside of the sick. While residing in the Scottish capital he made the acquaintance of Dr. Alison, whose lectures he attended, and an intimate friendship soon sprang up between the teacher and the pupil. Speaking of "Change of Type in Disease," in his Address to the British Medical Association, at Leamington, in 1865, Dr. Stokes said: "When I read these words of Alison—the best man I ever knew—it is with a feeling of wonder how it has happened that men should forget what reverence is due to his memory; whether we look on him personally as a man of science and a teacher, or at his life as an exemplar of that of a soldier of Christ. It was my good fortune to be very closely connected with him during my student days in Edinburgh, and to attend him by day, and more often far into the night, in his visits of mercy to the sick poor of that city, to whom he was for many a year the physician, counsellor, and support. This was forty years ago, and at that time he recognised the change. Often has he said to me, 'We cannot bleed this man; we must get him wine;' and the wine was got and given with an open hand so long as it was required."

In 1825 Stokes obtained the degree of Doctor of Medicine in the University of Edinburgh, along with his distinguished fellow-citizen, Sir Dominic J. Corrigan, Bart. Shortly afterwards he returned to Dublin, and became a Licentiate of the King and Queen's College of Physicians in Ireland on December 3rd, 1825. He at once commenced practice, and soon (January 7, 1826) succeeded his father as Physician to the Meath Hospital and County Dublin Infirmary. Dr. Whitley Stokes had himself been physician to the hospital only a little over seven years, having been appointed on the death of Dr. Thomas Egan, December 14, 1818.

On his election to the Meath Hospital William Stokes became the colleague of the illustrious Robert James Graves, who, in July of 1821, had himself been appointed Physician to the Hospital. This remarkable man was then twenty-five years of age; he brought to bear on the study of disease all the powers of a well-ordered, liberally-educated mind. He had taken the degree of Bachelor of Medicine in the University of Dublin in 1818, and had spent the following three years in visiting the medical schools of London, Berlin, Paris, Vienna, Copenhagen, and those of Italy. This was the education which, added to in-born gifts of no ordinary type, made Graves the man he was. In his travels he had become

acquainted with a new system of clinical teaching, which probably originated in Germany, and was introduced into Tuscany, where it was brought to perfection by a veteran Italian physician, for whom his country is but lately in mourning—Professor Maurizio Bufalini, of Florence. The superlative advantages of this system struck the observant mind of Graves, and he had scarcely been appointed Physician to the Meath Hospital when he introduced that method of clinical observation and instruction which has secured for him a lasting name—for his hospital and the Irish School at large a European—nay, a world-wide reputation.

In the eloquent words of his colleague, Dr. Stokes, "it may be safely said that up to his time clinical teaching, in the true sense of the word, was unknown, and that clinical knowledge was attainable but to the few who were forced by circumstances into observation at the bedside. To teach the general class of an hospital, in the fulfilment of a duty to them or to science, was an idea never realised by our hospital physicians or surgeons. Those great responsibilities of the British teacher of medicine, of which he has so largely and eloquently spoken, were unregarded, and so the student—unassisted, undirected—was left to grope his way as best he could. He was kept at a distance; no one cared to instruct him, to show him how to teach himself, to make him familiar with bedside medicine and the 'ways of the sick;' to exercise his powers of perception, to train his mind to reason rightly on the phenomena of disease—and, lastly, to make him learn the duty as well as taste the pleasure of original work. The impassable gulf which in that aristocratic era lay between the student and his so-called teacher was, by Graves, made to disappear, and for the first time in these countries was the pupil brought into a free, and full, and friendly contact with a mind so richly stored that it might be taken as an exponent of the actual state of medicine at the time—a mind ardent in research, fruitful in discovery, and yet no miser of its wealth, but ever ready to pour forth its riches to all, and for all, who were ready to receive them."

How well and nobly the writer of these words, one who was once Graves' pupil, then his colleague, ever his friend—William Stokes, maintained this system of clinical medicine, many can of their own experience

testify. How equally applicable to him are the words just quoted, generations of his pupils have good reason to know. There can be no doubt that it was this breaking down of the barriers between teacher and pupil which laid the foundation of modern clinical medicine. At the present day—so far from treating a pupil with surprised hauteur, should he be so indiscreet or so forgetful of etiquette as to ask a question—the clinical physician seeks to “draw out” the student, often *consults* with him on the patient’s case, and thus endeavours to make him think for himself. And so great gain results to both, for the teacher is often taught himself—*docendo discit*.

It was not to be expected that men like Graves and Stokes would let any improvement in clinical research escape observation. And so it came to pass that the then new science of “Auscultation” was made a subject of especial study in the Meath Hospital. Nor was it long before the physicians of the hospital gave to the profession the results of their inquiries into stethoscopy. In the 4th volume of the “Dublin Hospital Reports” they published a selection of cases to prove the utility of the stethoscope in the diagnosis and treatment of thoracic diseases. It is worth noting that the authors constantly speak of the “cylinder” in this paper, using that word as a synonym for stethoscope. Shortly afterwards (in 1828) Dr. Stokes delivered two lectures on the stethoscope, which he gracefully dedicated to the class of the Meath Hospital. These lectures prove how clearly the philosophical and medical mind of their talented author recognised in the little “cylinder,” already so spurned and spoken against, “a new and powerful weapon to aid him in the great battle with disease and death.” “His,” observes a writer in the *Dublin University Magazine*,^a “was no theoretic advocacy; even at the early period of which we speak the stethoscope was to him—what it has since become to the whole profession—the brightest light that guides the physician to an accurate diagnosis of disease.”

In the first of the two lectures, Dr. Stokes says: “I shall bring forward evidence to prove its (the stethoscope’s) extraordinary utility as an assistant in diagnosis; and I need not remind the class of the Meath Hospital that an accurate diagnosis is the only sure ground of judicious treatment. . . . What is the first step in the attempt to cure disease?

^a Dublin University Magazine. August, 1874. P. 151.

It is to distinguish it; it is to recognise it, obscured as it may be by a host of adventitious circumstances. This is, in fact, the great object of study. Diagnosis, then, is a paramount object, and we should, in the true spirit of philosophy, receive with thankfulness and avidity everything that can tend to remove its difficulties."

Ten years later we find Graves and Stokes jointly defending the stethoscope and auscultation from an irritable and unphilosophical attack by Dr. Clutterbuck. Their reply, which appeared in the *Dublin Journal of Medical Science*,^a is admirably conceived, and abounds with pointed yet delicate sarcasm. The truth is, these gifted men—one the author of a "Clinical Medicine," which has won from the illustrious Trousseau the highest encomiums; the other the author of the classical "Diseases of the Chest," and "Diseases of the Heart and Aorta"—wrote that reply, not to seek a controversy with Clutterbuck, but in the interests of the junior student "who might be deterred from studying an important and now indispensable part of his profession."

Some years previously, Stokes made a remarkable communication to the Association of the Fellows and Licentiates of the King and Queen's College of Physicians in Ireland on the light thrown upon the subject of Mesmerism by the study of nervous diseases. He showed how analogous the mesmeric state was to that condition of nervous excitement which prevailed epidemically in the sixteenth and seventeenth centuries, and which was regarded as a species of demoniacal possession. In these solemn words he decried mesmeric experiments—"Madness, convulsions, coma, tetanus, hydrophobia, epilepsy, and hysteria, are but manifestations of states of the brain and spinal marrow which we know to be analogous to the conditions of the induced or auto-mesmeric state, and as it has pleased the great Disposer of all events to spare us from the visitation of those epidemics of madness which occurred in the middle ages, it appears to be something worse than folly to reproduce even one of the forms of the malady indirectly, when our doing so can only amuse the vulgar or astonish the ignorant, while we risk the bodily or mental health of one of our fellow-creatures."

From the admirable biography in the *Dublin University Magazine*, August, 1874, which has been already quoted, and to which we here desire

^a First Series. Vol. XIV., p. 178, *seq.*

to express our indebtedness in the compilation of this memoir, we learn that in 1832 it fell to the lot of Dr. Stokes, together with the late Mr. Rumley, afterwards (1842) President of the Royal College of Surgeons, to report the first case of Asiatic cholera which occurred in Ireland. They were deputed to inquire into the cause of a sudden and mysterious death which had occurred at Kingstown, then little more than a seaside village. Neither Stokes nor his colleague had ever seen a case of the disease, yet the result of their inspection was that they pronounced the deceased to be the victim of the worst type of Asiatic cholera. Nothing could exceed the indignation of the inhabitants at this decision, which was—as they thought—a foul calumny on their healthy little town. The physicians escaped injury at the hands of an infuriated mob almost by a miracle; but the outbreak of the disease in various parts of Ireland within a few days settled the question, and verified the conclusion which had been arrived at.

Between the years 1832 and 1835, Dr. Stokes contributed a series of articles to the *Cyclopædia of Practical Medicine*, edited by Drs. John Forbes, Alexander Tweedie, and John Conolly. The subjects he treated of were—"Derivation," "Dysphagia," "Enteritis," "Gastritis," "Gastro-enteritis," "Inflammation of the Liver," and "Peritonitis" (the last conjointly with Dr. MacAdam).

In March, 1832, the first number of the *Dublin Journal of Medical and Chemical Science*—a bi-monthly periodical—appeared. The original projector and first editor of this Journal was Dr. (now Sir Robert) Kane. After the appearance of the first two or three numbers, Sir Robert Kane associated with him in his editorial capacity Drs. Graves and Stokes; and as the Journal assumed a more practical character, its management was chiefly conducted by the latter gentlemen, assisted for some time by the late Professor William H. Porter. Drs. Graves and Stokes continued as editors up to the year 1842, when their increasing avocations obliged them to resign the editorial chair of this successful periodical, which, as the *Dublin Journal of Medical Science*, has now attained its forty-seventh year.

The pages of the first series of the *Dublin Medical Journal* were enriched by many contributions from the pen of Stokes. Of these, his "Memoir on Pericarditis," published in 1834, is acknowledged to have "found pericarditis the most difficult, and left it the most easy of detection of any of the diseases of the heart;" his monograph, entitled "Observations on the Con-

dition of the Heart in Typhus," published in 1839, has ever since supplied the physician with reliable data on which to base the giving or withholding of wine in this disease; and, lastly, his "Clinical Observations on the use of Opium in large doses," published in 1832, served to promulgate the doctrine of Physiological Rest, and led to the scientific and rational treatment of peritonitis in perforation of the intestine.

In 1837 appeared the first of the triad of classical works on medicine with which the name of Dr. Stokes is associated—it was entitled, "A Treatise on the Diagnosis and Treatment of Diseases of the Chest." Of it Sir Dominic Corrigan wrote—"The present work is hailed with delight, both by those who love their profession as a science, and by those who, more humbly, but not less usefully, cultivating it as a practical art, seek in each new page that is presented to them the means of curing or alleviating disease." An able English reviewer spoke of it as "a work justifying the belief that medicine is really assuming the character of an inductive science."

In 1854 he gave to the world "The Diseases of the Heart and the Aorta," of which great work Dr. Sibson wrote—"Dr. Stokes' treatise, composed as it is of living pictures of disease, like his former work on "Disease of the Lungs," will last as one of the standards of experience, and take rank beside the works of Abercrombie, Cheyne, and Laennec."

In 1874 his "Lectures on Fever" were published. In them he dwelt especially on the important subject of the relation of the secondary affections of Continued Fever to the essential malady, and in the light of that relation discussed the question of treatment. This was, probably, the least happy of his writings, because so large a portion of the work is devoted to the consideration of a question long since answered beyond appeal in the affirmative—the question, namely, of the separate identity of typhus and typhoid fevers. To adopt his own words—"No one can deny that a normal case of typhus will show striking differences from a normal example of typhoid, yet that these differences belong rather to species than to genera, and that the principles of treatment of the two affections are the same, must, it appears to me, be admitted. In fact the study of the points of agreement between the two forms of Continued Fever under discussion will be more valuable than that of their differences."

At first it is not easy to understand how so accurate and admirable an

observer as Dr. Stokes failed at once and completely to isolate these two forms of continued fever. But it is probable that the admixture of cases of both fevers in the same wards, without any attempt at isolation of those suffering from typhus, led to the frequent occurrence of typhus in typhoid patients, and to the all but necessary confusing of the two diseases. But what shall we say of the splendid success which attended Dr. Stokes' investigations on the condition of the heart in fever, on the nature of the secondary affections, and on their relation to the primary essential malady? His philosophical mind recognised the essentiality of Continued Fever of whatever species, its subjection to periodic laws, whereby it tends to cease of itself, and the great principle of treatment dependent on this last-named peculiarity, which consists in *aiding the Vis medicatrix Naturæ*. Truly, if we write as the epitaph of Graves—"He fed fevers," we may write as that of Stokes—"He guided fevers."

Well does a recent writer in the *Irish Hospital Gazette*, while reviewing Dr. Stokes' work on Fevers, contrast and compare the labours of those three great men—Stokes and his colleagues, Graves and Hudson—when he says:—"This book, along with those of Graves and Hudson, fitly completes the triad of speculative, practical, and philosophical works on fever, the produce of the triad of illustrious physicians of whom it is the enviable fortune of a single hospital to be proud."

Nor did Dr. Stokes' literary labours end here. The gifted editor of Graves' "*Studies in Physiology and Medicine*," the eloquent biographer of Graves, the sympathetic author of "*The Life of George Petrie*"—he has indeed established an indisputable claim to be regarded as a master of English. His public discourses—at the Meath Hospital, before the University of Dublin, and to the British Medical Association—illustrate "the majestically impressive diction, with the serene calm of philosophic sublimity," of which Dr. Foot, now Senior Physician to the Meath Hospital, spoke in his recent Introductory Address as so characteristic of Dr. Stokes' utterances.

To the *Dublin Journal of Medical Science* for August, 1847, the late Dr. Cusack and Dr. Stokes contributed a paper on the "Mortality of Medical Practitioners from Fever in Ireland." In the year 1843 the Medical Charities Bill had been brought forward, and the authors, who gave most valuable and convincing evidence before a Select Committee of

the House of Commons, state in this paper that they were not without hope that some recognition of the labours and risks of their professional brethren in Ireland would have appeared in the Act of Parliament; for "we felt," say they, "that the amount of these labours and the nature of the risks were not known to the Government."

On the authority of Inspector-General Marshall, the authors showed that from January, 1811, to May, 1814, a period of 41 months, the mortality of combatant officers in the Army was about $10\frac{1}{4}$ per cent. from battle and disease combined. But the mortality of the medical practitioners in Ireland, during a period of 25 years ending in 1843, was about 24 per cent. The appalling fact appeared that within this period one out of every 2.29 deaths of medical men in Ireland occurred from fever—almost always typhus. Happily this state of things no longer exists; yet we cannot but endorse the words with which this paper closes: "The Government, while, on the one hand, it seeks to remove the causes of the spread of fever in Ireland, should, on the other, provide that the poor and destitute man shall not receive inferior or insufficient medical assistance; and reward with no niggard hand, and encourage by every means in its power, the exertions of a class of gentlemen, we fearlessly assert, the best educated, the least prejudiced, the bravest, and the most philanthropic in the community."

With Preventive Medicine—the last outcome of medical thought and progress—the name of Dr. Stokes is closely identified. He was one of the earliest, ablest, and most disinterested advocates of the doctrines of State Medicine. For many years he used all his powers of mind, and eloquence, and writing, to promulgate and advance these doctrines. It was to him that the founding of a diploma in State Medicine in connexion with Trinity College, Dublin, was mainly due. In a "Prelection" before the University of Dublin in April, 1872, he said:—"Preventive Medicine embraces everything, as is well shown by the Regius Professor of Medicine at Oxford, which relates to the physical and moral well-being of our fellow-men, so that it has to contend with all moral, social, and physical evils. Ignorance, selfishness, the grinding of the poor, the consumption of human life, like fuel, for the production of wealth, vicious indulgence, and everything that deteriorates the body, and with it the mind, come within its extended scope. Its object is the health, and

therefore the happiness and prosperity of man—its instruments are science and common sense, with rules plain and patent to all, so that it promises to be the noblest pursuit yet offered to the human intellect, and he would be a bold man who would dare to limit its results or to predicate its triumphs.”

These eloquent words are from the peroration of the same Address:—

“A time may come when the conqueror of disease will be more honoured than the victor in a hundred fights. The time may come when no man for his own ends or for his profit will be permitted to damage the health or the well-being of his neighbour or of his servant, nor the prisoner have to suffer through the ignorance or the indifference of his jailer, while the emigrant with his loved ones will be protected from disease as he expatriates himself from the land of his birth.

“The gifts to man from Heaven—pure air, pure water, bright light, and wholesome food—will be more freely shared in, and the moral and physical evils of overcrowding, and the consequent guilt, the shame, the pestilence, will disappear. The artisan will be taught the dangers of his particular calling, and—so far as law and public opinion go—be protected from them, whether he labours in a hot room, amid the roar of machinery, or deep in the earth where he has to work in passages carved by himself of little more than two feet in height, inhaling the smoke of gunpowder and particles of silex till his working life comes to an untimely and miserable end.”

In this memoir it would be impossible to do more than mention some of the many distinctions which were showered upon the author of these words. He possessed Honorary Degrees of four great British Universities—M.D., of Dublin (1839); D.C.L., of Oxford; LL.D., of Cambridge; LL.D., of Edinburgh (1860). He was a Fellow of the Royal Society. In 1867 he was President of the British Medical Association. In 1874 he became President of the Royal Irish Academy. On October 18 (St. Luke's Day), 1828, he was elected an Honorary Fellow of the King and Queen's College of Physicians. He became a Fellow of the College in 1839, and filled the posts of Censor and Vice-President in 1848. In 1849 he was chosen President of the College, to which position of dignity he was again appointed in the following year. The College Mace, said to be the most elaborate in the United Kingdom, was designed

by his friend, Frederic Burton, during this second presidency, and was finished in 1853. Around its base runs this legend—"Guilielmo Stokes, Præside A.D. MDCCCL.," with the crest of Dr. Stokes. In 1845 he succeeded his father, Dr. Whitley Stokes, as Regius Professor of Physic in the University of Dublin. In 1854 Her Majesty the Queen appointed him one of her Physicians in Ordinary in Ireland, and in 1858 extended to him a further proof of her Royal favour and appreciation of his great talents in nominating him to be the Representative, for Ireland, of the Crown on the General Council of Medical Education and Registration. This post he filled until May 1, 1877, when failing health obliged him to resign the trust. To his great gratification, his friend and former colleague at the Meath Hospital, Dr. Alfred Hudson, was nominated his successor by Her Majesty. In addition to these titles and honours, Dr. Stokes possessed the rare distinction (shared by but two distinguished men of science in Ireland—Dr. Humphrey Lloyd, Provost of Trinity College, Dublin, and Dr. Romney Robinson, the astronomer, of Armagh Observatory) of the Order of Merit of Frederick the Great, presented by the Emperor of Germany. The Prussian Order "*Pour le Mérite*" was originally granted only for military services in the field. In 1842, however, King Frederick William IV. made the Order a civil one, and granted it for proficiency in science and art. Among the "*Ausländische Ritter*," nominated in 1875, was "*Wilhelm Stokes, Professor an der Universität zu Dublin.*"

On March 16, 1876, a marble statue of Dr. Stokes, from the studio of the late lamented Foley, was unveiled, and formally handed over to the King and Queen's College of Physicians. On that occasion Dr. Hudson delivered an eloquent eulogium, to which we are largely indebted in writing this memoir. In that address Dr. Hudson said: "The sculptor has most happily given this expression of thought to the face and attitude of the statue. It seems to pourtray one who

"Deep and slow, exhausting thought,
And living wisdom with each studious year,
In meditation dwelt with learning fraught."

"Not that Dr. Stokes' life has been one of learned leisure. On the contrary, having at an early period won the confidence of the profession and the public, his life has been one of incessant labour, and his contributions to medicine have been made under the pressure of constant professional

engagements. When to this consideration we add the fact that Dr. Stokes is no mere drudge, no lover of work for work's sake, but a man in whose mental constitution it were hard to say whether the intellectual or the æsthetic predominates; a man gifted with a rich and poetic imagination, with exquisite taste for art—witness his life of Petrie—and with a rare capacity for the enjoyment of literary and social intercourse, our gratitude and admiration are enhanced, and we feel that,

“ ‘ More honoured still
Should be the labour and the will.’ ”

On the same occasion Dr. Hudson spoke of the boon which Dr. Stokes, in conjunction with Sir D. Corrigan, the late Professor R. W. Smith, and other eminent men, conferred upon successive generations of students and practitioners by the institution of the Pathological Society of Dublin, the first of the kind in these countries. In Dr. Hudson's words—“ The object and scope of this Society might be said to be a reflex of Dr. Stokes' character as a pathologist; not devoted to any school or system, but eclectic; neither regarding morbid anatomy as its first object, but as subsidiary to pathology considered in its widest sense, and embracing the history, symptoms, diagnosis, and treatment of each case of disease submitted to the Society. To this Society Dr. Stokes, as Honorary Secretary, devoted, for many years, much time and labour, and made numerous valuable and important communications. These, like his other published works, present the combination of accurate observation and clear insight with mature reflection. No mere observer and recorder of random facts, he seems always to have kept in view the ancient maxim—‘ *Ars tota in observationibus, sed pendæ sunt observationes.*’ ”

In April 1875, Dr. Stokes had resigned the Physicianship to the Meath Hospital, which he, honoured and beloved, had held for nearly fifty years. Advancing years and failing health were Nature's pleas for rest from toil; and civic-crowned, the “ conqueror of disease ” throughout half a century, laid down his arms to enjoy a well-earned repose in a green old age. More than two years and a half rolled by ere the final stroke came. These years brought ever-increasing weakness, against which at first the great mind strove at times. But afterwards unrest gave place to a peaceful calm, and so he passed away—to use the words in which he himself spoke of another, the late Dr. Josiah Smyly—“ after a long and splendid life of

deserved and almost unexampled success, with 'honour, love, obedience, troops of friends.'"

On Friday, January 11, 1878, the remains of Dr. Stokes were borne from his villa, Carrig Breac, Howth, to Saint Fintan's churchyard, near Sutton, on the western slope of the Hill of Howth—

"Where, mingling with the wreckful wail,
From low Clontarf's wave-trampled floor,
Comes, looming up the burthened gale,
The angry sandbull's roar."

In accordance with the express wish of the family the funeral was private, and, accordingly, the King and Queen's College of Physicians, the University of Dublin, and the Royal Irish Academy were not officially represented on the occasion. Notwithstanding, most of the leading members of the profession in Dublin and its vicinity were present, and some fifty students of the Meath Hospital walked in procession before the coffin, which was borne on the shoulders of the neighbouring peasantry. On reaching the little churchyard, eight students carried the coffin to the grave, where the concluding portion of the beautiful burial service of the Church of Ireland was read. And so, on a bright, frosty morning, all that was mortal of William Stokes was laid in the grave, near his own loved ones, under the ivy-clad ruins of Saint Fintan's church. This touching simplicity in its *requiem* was a fitting sequel to a life of genius, virtue, and humanity.

A model and diligent student, he in time became a painstaking and successful teacher—the sympathising friend, the prudent counsellor, and the ardent well-wisher of every one of his "fellow-students," for so he called his pupils. But he was more than this. Those who have seen Dr. Stokes at the bedside of the sick, know how gentle, how refined, how kindly was his bearing towards the patient. Amid all the ardour of clinical observation and research, he never for one moment forgot the sufferer before him—no thoughtless word from his lips, no rough or unkind action ever ruffled the calm confidence reposed in him by those who sought his skill and care. In many eloquent lectures delivered in the Meath Hospital he inculcated these Christian lessons of charity and thoughtfulness; and so, by precept and example, he strove to teach the duties of a true and God-fearing physician.

J. W. MOORE.

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MEDICAL SCIENCE.

MARCH 1, 1878.

PART I.

ORIGINAL COMMUNICATIONS.

ART. VI.—*The Principles of Electro-Therapeutics.*^a By WALTER G. SMITH, M.D., F.K.Q.C.P.I.; Assistant-Physician to the Adelaide Hospital.

LECTURE I.—PHYSICAL.

Synopsis.—Historical Sketch. Definitions:—Potential—Electro-motive Force—Intensity—Du Bois Reymond's Law—Resistance—Ohm's Law—Density. Forms of Electric Currents utilised in Medicine:—I. GALVANIC (VOLTAIC) CURRENTS. 1. Continuous. 2. Interrupted: (a) Direction uniform—(b) Direction alternating. II. INDUCED (FARADIC) CURRENTS—3. Electro-magnetic: (a) Extra-current of primary coil—(b) Alternating Currents of secondary coil. 4. Magneto-electric Currents. Graphic representation of Current-curves. General comparison of Galvanic and Induced Currents. ELECTRO-MEDICAL APPARATUS.—(a) Galvanic Batteries—(b) Induction Machines (Inductoriums). Electrodes or Rheophores—Commutator. Propagation of Currents through conductors of various forms. Laws of Transmission and Localisation of Electric Currents in the Human Body. Principles of localised Electrification.

MR. VICE-PRESIDENT, FELLOWS, AND GENTLEMEN,—In selecting as the subject of these lectures the principles upon which the art of electro-therapeutics rests, several considerations urged themselves in its favour.

The scientific practice of medical electricity is a branch which

^a Two lectures delivered before the King and Queen's College of Physicians in Ireland, on February 4th and 11th, 1878, being the first and second of the Annual Scientific Lectures for 1878.

has emerged from a state of infancy into one of vigorous growth and fair proportions within the recollection of the junior members of the profession—one, moreover, whose rapidity of development within very recent times has been so surprising that busy men can scarcely find time to follow its progress; and further, one whose practical importance is so pressing that it is the bounden duty of every clinical instructor to teach it, and it is the interest of the physician and surgeon alike, as it is a necessity of the physiologist, not to neglect a just appreciation of its value. The title of *principles* of electro-therapeutics was advisedly chosen, because, as it appears to me, it is ignorance of first principles which is the cause of much of the slipshod theory and extravagant practice that have so tarnished the history of medical electricity.

Into the details of the practice of electro-therapy, and the indications for its use in special diseases, it would be quite impossible to enter in the time allotted to these discourses.

Upon the history of electro-therapeutics, which is essentially bound up with the cardinal physical discoveries made in the domain of electricity, and with which, step by step, it has advanced, I will dwell but a moment. The most remarkable circumstance connected with it historically is the immense space of time—more than two thousand years—which elapsed from the first recognition of electrical power before a single new fact was added, or even an experiment made.

It was not until the year 1746, after the accidental discovery of the Leyden phial by von Kleist, Dean of the Cathedral in Comin, that the study of electricity became general, and invited the attention of other than philosophers (Cavallo on Electricity, 3rd Ed., 1786). During the latter part of the 18th century the new art was promoted by such men as Cavallo, Humboldt, Franklin, Volta, Ritter, and Pfaff, but any general adoption of it in practice was hopelessly clogged by the cumbrous nature of the apparatus, and by the uncertainty of the methods of application.

In 1786 and following years, Galvani immortalised himself by his discoveries in animal and in contact electricity; and the year 1799 is memorable for the construction, by Volta,^a of the first voltaic battery—the parent, it may truly be said, of all the later advances in electrical science, and itself a discovery which “has done perhaps more than any historical event towards the progress

^a The celebrated letter of Volta announcing his great discovery to Sir J. Banks (20th March, 1800), is preserved in the Philos. Trans., 1800, p. 402.

of civilisation and the prosperity of mankind." In the same year Nicholson and Carlisle accomplished the electrolysis of water, and thus opened the way to the surgical applications of electricity.

Still the time had not come for any fruitful application to scientific medicine of these pregnant discoveries, and the first three decades of this century are barren of any important advance in the art of electro-therapeutics. In 1830 and succeeding years, Faraday dazzled the scientific world by his great researches^a in the sphere of induction currents, and at length materials were at hand wherewith to begin a successful investigation into the action of electric currents upon organised bodies—a study which has borne such rich fruit in the hands of Matteucci, and, above all, of Du Bois Reymond. The latter physiologist was the creator of a scientific method of study of electro-physiology.

Soon also appeared the apostles and fathers of electro-therapeutics in Remak (1855-65) and Duchenne (1847, *et seq.*), who, despite of their polemical bias—the one for the galvanic current, and the other for induction currents—undoubtedly laid the foundations, deep and true, of our present system of electro-therapeutics, the physical aspects of which have been elaborated by Helmholtz, Rosenthal, Fick, and others. Before Duchenne there was no such thing as a system of electro-therapeutics—he it was who brought order out of chaos; and it may justly be asserted that the past quarter of this century has witnessed the birth and growth of electro-therapy as an applied science.

In the course of these lectures there will be frequent occasion to employ some physical terms to which it is very necessary that no ambiguity should attach, and I will now ask your attention for a few moments to the meaning in which each of them will be used.

The flow of a current of electricity may be compared to that of a stream of water descending from a higher level to a lower, and the idea, in either case, necessarily involves a relative condition of two points. The electric condition or level of the earth is taken as zero, and if the electric condition of another body be at a higher level than that of the earth there will be a transfer of electricity from that body to the earth; and, if at a lower level, from the earth to the body, until equilibrium is restored. Positive electricity corresponds to higher potential, and negative electricity to lower potential (Plate I., Fig. 1).

We have here an analogy to the comparison of the relative

^a The *extra-current* was not discovered until 1835.

position of two places as so many feet above or below the sea level; or, an electric current is comparable to the flow of a stream of water through a channel of uniform fall and section.

This difference of electric condition of two bodies is now usually termed difference of *Potential*, or difference of height, so to speak. There can be no current either of water or of electricity without a difference of level, and the greater this difference, the more energetic will be the *Electro-motive Force* (E. M. F.) of any pair of dissimilar substances.

The *Intensity* of a current is the quantity of electricity which flows through a conductor in a unit of time, and is analogous to the velocity with which water flows through a uniform channel.

Connected with this quality of a current is one of the fundamental laws of the action of electricity on the human body—viz., Du Bois Reymond's law of stimulation, which may be thus expressed:—

A motor nerve or muscle is excited, not so much by the absolute intensity of an electric current, as by the *variation in intensity* which the current undergoes from moment to moment, and the irritation is more energetic the greater these variations are in the unit of time, or the more rapidly they succeed each other, their magnitude remaining constant.

This law holds only as an expression, in the most general terms, of the conditions of excitation.

The intensity of a current is evidently conditioned by the primary E. M. F.—*i. e.*, the quantity of electricity originally liberated, and by the *Resistance* which the current meets in its path. For as a stream of water cannot flow through a pipe without encountering the retarding effect of friction, so there is no substance which does not oppose more or less impediment to the passage of an electric current.

The conducting power or conductivity of a body is, therefore, the reciprocal of its resistance (R.) The resistance offered to the transfer of electricity across the cells of a galvanic battery, or the coils of an induction spiral, is termed internal resistance; all other resistance in the circuit is termed external resistance. The difference in the resistances of the various tissues of the body bears directly upon the practical application and localisation of electricity.

The highly important and pregnant law which binds together

the three principal terms just defined—viz., resistance, intensity, and electro-motive force—was discovered by Ohm in 1827. It is usually expressed thus:—The intensity is equal to the E. M. F. divided by the resistance (*i.e.*, $I = \frac{E}{R}$), or, in other words, for a conductor in a given state, the E. M. F. is proportional to the current produced—*i.e.*, the ratio of the E. M. F. to the current is independent of the strength of the current.

A clear appreciation of the applications of Ohm's law is essential to the electro-therapeutist, and some valuable practical consequences flow directly from it. Imperfect acquaintance with this fundamental law, which cannot be further pursued here, has led to erroneous views—*e.g.*, on the difference between the physiological effects of the induced currents of the primary and secondary coils—which prevailed for years, and have scarcely yet been banished from treatises on electro-therapeutics.

In speaking of electric currents it is very necessary to avoid an error, common in medical writings, of confounding the terms density and intensity. By the *Density* of a current is understood the quantity of electricity per square unit of section. Therefore, for equal intensities of current, the greater the section of the conductor, the less will be the electric density. Thus, if a current of constant intensity be passed successively through two good conductors of the same material, one being twice the length and twice the sectional area of the other (the resistances, then, being equal) the current will evidently be in a state of higher density on the smaller of the two conductors.

The variations in the density of a current have much to do with the rational use of electricity in medicine. For it is clear that in physiological actions, it is of material importance whether the same quantity of electricity be distributed over a larger or smaller surface. In the latter case the density will be greater, and the physiological action, therefore, more considerable.

Again, since the human body is composed of a vast number of conductors—*e.g.*, bone, muscle, nerve, &c.—heterogeneous in nature, and of unequal conductibility, it follows that a current starting from any point on the surface, and passing inwards, must split up into a number of branches, forming a system of "divided currents." The intensity of the current in each conductor will be inversely proportional to the resistance of the conductor or directly proportional to its conductivity. Divided or derived currents are precisely those which meet us in the study of electro-therapeutics,

and may be likened to the branching of blood-currents into a number of different arteries from a common trunk; the larger the vessel—*i. e.*, the less its resistance—the more blood will flow through it. To take a familiar illustration:—If two electrodes, placed on the surface of the body, be touched by the moistened fingers of a second individual, part of the current will be diverted into his body, and communicate to it a feeble derived shock.

Let us now inquire what are the forms or modes of electric currents which are utilised in medicine.

Without disparaging the mass of well-attested evidence accumulated within the past century of the beneficial effects of static or frictional electricity, we are quite justified in holding that its place is better taken by other forms of electricity, and, leaving it aside, we may conveniently arrange the varieties of electric current under the following heads:—

I. GALVANIC (VOLTAIC) CURRENTS.

1. The *Continuous* or constant galvanic current—developed in one of the many kinds of galvanic battery, each cell of which consists essentially of a pair of dissimilar plates in an acid or saline solution.

The course of the current, when lasting for a sensible time, is shown in the diagram^a (Plate I., Fig. 2). It rises rapidly from zero to a maximum intensity, remains constant for a time, and, on breaking the circuit, falls suddenly to zero.

This form of current exerts well-marked chemical, thermal, and electrolytic effects, on account of its uniform direction, and it is the only one utilised in surgery for galvano-caustic and electrolytic purposes. The chemical and polarising effects of the current are of great significance in a comparison of the relative physiological actions of induced and galvanic currents.

The action of this form of current, in its relation to the therapeutics of nervous diseases, may be broadly characterised as (a) sedative, and (b) modifying or alterative.

2. The *Interrupted* galvanic current—*i. e.*, an intermittent series of brief currents, produced by a toothed wheel, a trembler, or a hand-interrupter. Its course, as appears from the diagram, differs graphically from the first form only in abbreviation of the period of constant intensity.

^a In each of the diagrams the horizontal base line represents duration; the vertical height, intensity.

FIG. 1.

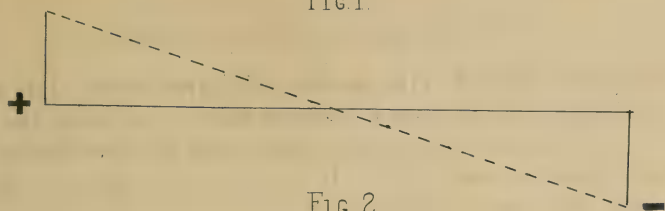


FIG. 2.

*Voltaic Currents.
Continuous.*



FIG. 3.

*Interrupted.
Uniform Direction.*

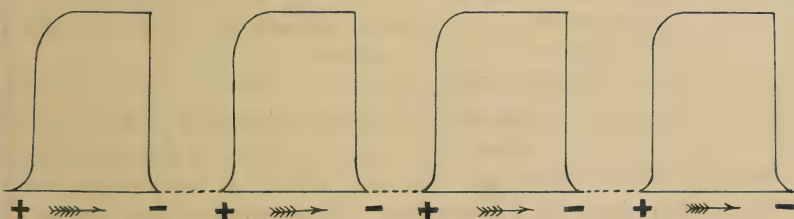
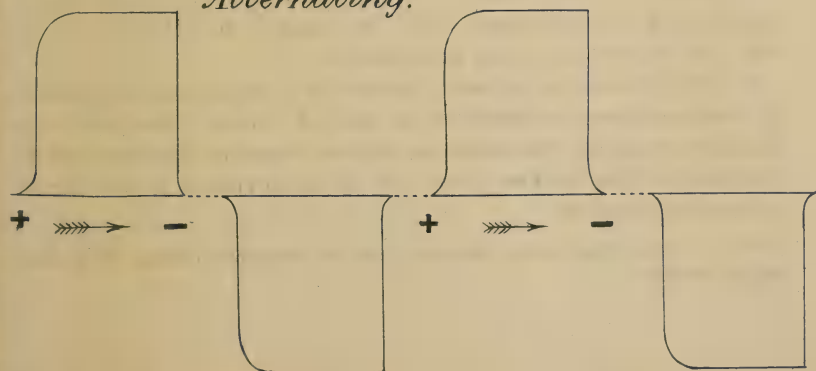


FIG. 4.

Alternating.



But now we must distinguish two important subdivisions.

(a.) Intermittent currents, the direction of the current remaining uniform. The chemical and electrolytic effects will be still maintained (Plate I., Fig. 3).

(b.) Intermittent currents, alternating in direction (Plate I., Fig. 4).

The physiological effects of the alternating currents (*voltaic alternatives*) as a stimulus to nerve and muscle are much the more considerable, and the physical reason for this is that the difference of irritative action of the two poles is doubled. For, if we close the circuit of a battery, *e.g.*, of six cells, we have the effect of a current which rises from 0 to 6; while on breaking the circuit, the current rapidly falls from 6 to 0. In each case the difference = 6. But when alternation is made, the current falls at one pole from + 6 to - 6, while at the other pole, it rises from - 6 to + 6. Thus at both poles the difference = 12 (Brenner). Therapeutically the action of an interrupted galvanic current is essentially of a stimulating or exciting nature.

It is, of course, incorrect to assume, as is often done, that interrupted currents are synonymous with induced currents. Induction currents are necessarily intermittent, galvanic currents may or may not be so. In Therapeutics the term *Galvanisation* properly means only the application of either a continuous or interrupted galvanic battery current; although we still too often hear of a patient having been galvanised when he really was faradised.

II. INDUCED (FARADIC) CURRENTS.

Of these we have two subdivisions corresponding respectively to the inner (primary) and outer (secondary) coils of an ordinary electro-magnetic apparatus—*i. e.*, one in which the source of power is a galvanic current. The currents in the coils are materially reinforced by the presence of a *core* of soft iron wires which becomes an electro-magnet so long as the current circulates. [Experiment shown.]

3. The *Extra-current* induced in the primary coil.

The course of this current, as exhibited in the diagram (Plate II., Fig. 1), rises somewhat gradually at the closure of the circuit, and falls abruptly on breaking the circuit, because the induced current then runs in the same direction as the parent battery current, and experiences no retardation.

Since the extra-current induced on making contact finds for itself,

necessarily, a short circuit in the battery, it evidently cannot manifest itself physiologically. Therefore we have to take into account only the opening extra-current, which is uniform in direction, and thus is directly comparable to an interrupted voltaic current of constant direction. Herein lies a noteworthy difference, not always sufficiently heeded, between the course of the extra-stream, and that of the next form of induction current.

4. The *Alternating currents* induced in the secondary coil of an electro-magnetic apparatus.

Their course, graphically laid down (Plate II., Fig. 2), shows that the current induced on making contact is opposite in direction to the closing extra-current; and, being retarded, it gradually attains its maximum, and gradually falls, and so its physiological action will be relatively weak. But the current induced on breaking contact is in the same direction as the opening extra-current; and, not being retarded, it immediately attains its full strength, and rapidly falls. Hence, by Du Bois Reymond's law, the physiological action of the opening current is greater than that of the closing current, because the variations in its intensity are more rapid and extensive. [Experiment shown on the muscles of the forearm.]

But it is particularly to be observed that the total quantity of electricity is the same in the closing and opening currents, as is indicated by the equality of the areas enclosed within the two curves. That is, the difference in physiological effect is dependent on a difference in the distribution of similar quantities of electricity.*

It should be noted that the excitation determined by secondary induced currents is not a simple one, for it is composed of two shocks which succeed each other so rapidly that they merge one into the other, physiologically speaking.

From this we see that the extra-current, which gives a single excitation, is correlated to subdivision (a) of galvanic currents; and the secondary induced current, which gives a double excitation, to subdivision (b), a relation which has been too much lost sight of. The duration of induced currents is extremely short, that of the opening secondary current being probably about one-third of the closing current.

That is to say, with two currents of equal quantity, their dura-

* By an ingenious arrangement due to Helmholtz the intensity of the closing and opening shocks can be equalised, but it has not come into use in the construction of apparatus for medical practice.

FIG. 1.

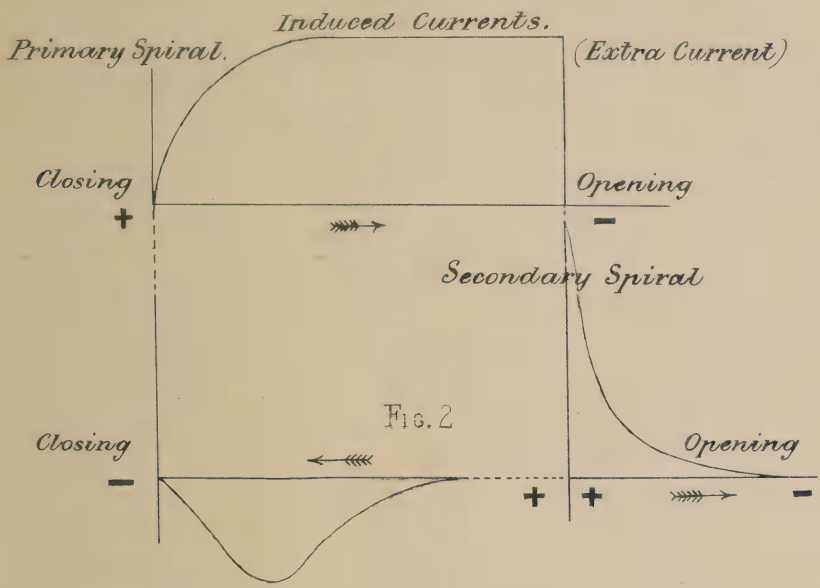


FIG. 3.

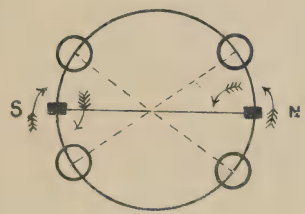
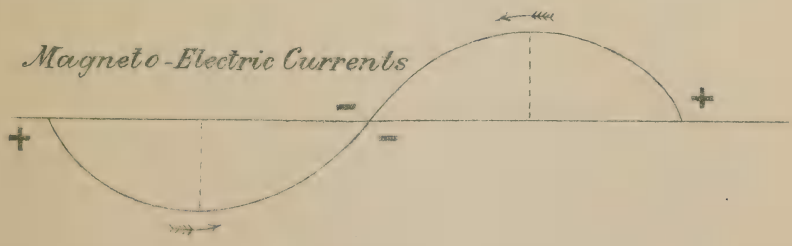


FIG. 4.



tion may be very different, and consequently they may exert very different physiological actions. The almost infinitely short duration of induced currents is the chief reason why, in certain cases of paralysis (*e.g.*, facial palsy), induced currents lose their effect, whereas slowly interrupted galvanic currents preserve their action upon the paralysed muscles.

Another important factor, in a physiological view, is the rapidity of intermission of the shocks. In the ordinary medical induction machines the rate can be made to vary from about 100 to 500 times in a second.^a

The chemical, thermal, and electrolytic effects of secondary induction currents upon living tissues are practically unimportant, owing to the momentary duration, and oscillations in direction, of such currents.

A striking proof of the non-electrolytic action of faradism is given by Ferrier. He states, "I have in my possession the brains of monkeys and other animals on which experimentation by the induced current was maintained for many hours, which, with the exception of some degree of hyperæmia, consequent on exposure as much as stimulation, are entirely free from structural lesion." (*Functions of the Brain*, 1876, p. 131).

5. *Magneto-electric currents*—*i. e.*, currents induced in a coil of wire by a permanent steel magnet, the arrangements being those of an ordinary rotary machine (often incorrectly called electro-magnetic machine). [Experiment shown.]

The course of the currents induced in a coil of wire, surrounding a soft iron armature, during a complete revolution before the poles of a magnet (Plate II., Fig. 3), is shown in the diagram (Plate II., Fig. 4), from which it appears that the induced currents corresponding to the appearance and disappearance of the magnetism of the iron armature are of equal intensity. This is a very different state of things from the course of electro-magnetic induction currents, where the relative intensities of the opening and closing currents are nearly as 3:1.

The therapeutic application of any of the forms of electric current (3, 4, or 5) is termed *Faradisation*, which is restricted to the use of induced currents alone.

From these considerations we perceive that electric currents differ one from the other in certain features—*viz.*, direction,

^a In the enormous coil recently employed by Mr. Spottiswoode for experimental purposes, the trembler gives 2,500 breaks in a second.

intensity, density, duration, and quantity as measured by thermal or chemical effects—to which may be added, as a most important factor in a physiological or pathological point of view, greater or less rapidity of intermittence.

Now, I wish to emphasise particularly that there are no mysterious peculiarities of voltaic battery currents (continuous or interrupted), as distinct from those of induction currents; and further, that between the different species of induced currents there is no *essential* difference in action, and none which cannot be explained by the known physical properties of the currents. We may accept this principle without disputing Du Moncel's assertion, that the reciprocal relations between the nervous system and electrical actions are not yet sufficiently well known to enable us to explain *à priori* all the observed phenomena.

The next subjects that will engage our attention are the instruments and appliances by which these various modes of electricity are generated, and by which they are made available for practical purposes. On this topic I shall not attempt any general survey, and will refer only to those forms of apparatus which appear to be deserving of general adoption.

I. Galvanic or Voltaic Batteries.

The varieties of cell usually employed in medical practice, that furnish a constant or nearly constant current, are either some form of Daniell's or Bunsen's cell, or one of the more recent peroxide cells (*e. g.*, Leclanché). In all of these the zinc terminal is the negative pole. The solution employed has little effect on the E. M. F. of a cell, but has a great effect on the resistance; and the increase in strength of a current, by using dilute acids or saline solutions in the cell, is due mainly to the change in the total resistance of the circuit, rather than to increase in the E. M. F. Local action (*i. e.*, useless oxidation) is much increased by diminishing the resistance of the fluid—(F. Jenkin.)

Now the essential point in selecting a particular form of cell is the *kind of work* required of it, and no one form of cell is the best possible for all cases. One cell differs from another, according to its construction, in electro-motive force or potential, in interior resistance, in constancy, and in enduring power or permanency; and the practical object is to get, for a given kind of work, the best combination of the most suitable elements.

In the majority of circumstances, except among specialists, a medical battery is required for occasional use only, and for short

periods of time. The great desiderata are, that a battery, when out of action, should need as little attention as possible, that it should be ready for instant use, be inoffensive, enduring, and not too costly.

There can be no doubt that Daniell's cell, discovered more than forty years ago, is, in any one of its more recent modifications (Muirhead, Siemens and Halske, &c.), the most constant combination yet known, and for certain purposes—*e.g.*, in large institutions, and in the hands of specialists where due care will be paid to its replenishing—it holds the first place. But for ordinary practice, with the exception of galvano-caustic applications, I have no hesitation in saying that the Leclanché element, invented ten years ago, has not yet been surpassed, and from seven years' experience it appears to answer every demand that can reasonably be made upon it, even although, being a single fluid cell, it cannot give either a constant electro-motive force or a constant resistance; for the E. M. F. of such cells diminishes very rapidly as soon as the circuit is closed, owing to a reciprocating or opposed E. M. F., produced chiefly by the presence of free hydrogen upon the negative plate. But since, in medical use, a battery has frequent and long intervals of repose, the E. M. F. is gradually restored by rest, partly through the liberation of the hydrogen, and partly by its recombination with oxygen.

The Leclanché element consists of a porous cell filled with a coarsely granular mixture of charcoal and black oxide of manganese (pyrolusite), from which a rod of carbon projects (+ pole). The porous cell stands in a glass cell containing the zinc plate or rod (— pole), immersed in a saturated solution of sal-ammoniac. The Leclanché battery is sufficiently constant, requires little attention, local action is at a minimum, and it possesses extraordinary enduring power. A modification of the Leclanché cell by Clamond, which consists in the substitution of peroxide of iron for peroxide of manganese, also promises well; and one such battery which I have had for the past year furnishes a convenient, portable, and satisfactory apparatus, especially for out-door practice.

With all batteries, care should be taken to use them equably, so as to avoid premature exhaustion of a few cells.

As to the number of cells requisite to form an efficient battery different opinions are held; and some authorities teach that it is needful to have 100 cells at command. For the electrolysis of small nævi a series of 10 cells will suffice; to procure the sedative

effects of electricity—*e. g.*, in neuralgia—the number should range up to 20 cells; and for the full stimulating effects of a galvanic current, it will be necessary to have 30 or 40 cells at disposal. The cases are few in which the want of a larger number than 40 cells will be experienced, and most requirements will be satisfied with 30 cells.

From a simple consideration of Ohm's law, two important practical consequences directly follow:—

(*a.*) With a large external resistance—*e. g.*, the human body, a considerable number of cells is necessary, which may be of small dimensions.

(*b.*) With a small external resistance—*e. g.*, a metallic wire (galvano-cautery), a few cells of large surface are requisite.

Although for medical purposes the size of the plates of the cell is of comparatively little importance, it is not without some significance, and an objection to very small elements is that they polarise rapidly, and thus yield an inconstant current. The most familiar combinations of very small elements are seen in Pulvermacher's chains. Of these the small bands are mere toys; the large bands are extremely inconstant in action, but are sometimes serviceable in the absence of better appliances, and should be used by the physician according to the methods followed with ordinary batteries. Frequently they are employed in an absurd manner, and I have repeatedly known troublesome cauterisation of the skin, with subsequent unsightly scarring, to be occasioned by their injudicious use. The notion, moreover, of applying permanent continuous currents, while it appeals to the imagination and credulity of the public, is not based upon sound physiological principles, and is rejected by all the best electro-therapeutists.

II. Faradic or Induction Machines.

A great variety of excellent forms of induction apparatus has been brought out, and it is easier to procure a good inductorium than a reliable galvanic battery. For ordinary use, and especially as regards portability and simplicity of management, a coil worked by a zinc-carbon cell with bichromate of potash solution is thoroughly satisfactory.

The bichromate cell is an energetic electromotor—*i. e.*, it has a high potential and small resistance, and although it comparatively soon loses in power, this is a trifling objection, since it is easy to renew the solution.

Stöhrer's larger apparatus I have also used for many years with

considerable satisfaction, and his instruments are in much favour on the Continent. The arrangements for the trembler in this machine are needlessly complex. It is quite possible that the introduction of a thermo-pile as an electromotor for inductoriums, will before long render such instruments even more portable, cleanly, and convenient to work. Although induction machines are usually provided with two coils, a primary helix of thicker wire, and a secondary helix of longer and finer wire, yet in the large majority of cases, the current from the primary coil is amply sufficient for all purposes. It is practically immaterial whether we employ a stronger primary or a weaker secondary current, but the secondary current labours under the disadvantage of being more painful.

If magneto-electric inductoriums be still preferred, it is imperative that the means for graduating the current be more accurate than that commonly provided, and some such arrangement as that used by M. GaiFFE should form part of the apparatus.

Of the accessories of electro-medical apparatus little need be said. For ordinary purposes of neuro-muscular stimulation, the electrodes or terminals of the conducting wires should be attached to knobs or discs of metal or carbon of various forms, covered with chamois or soft flannel, in preference to cupped sponge-holders. The former are not only much more convenient and cleanly to use, but also offer less resistance to the passage of the current than sponge; and cylindrical sponge-cups may be entirely rejected.

Inasmuch as in the case of galvanic currents we employ both continuous and interrupted currents—and the effects produced at the moments of closure and opening of the current, and during its passage, must be carefully distinguished—it is necessary to be provided with artificial means of interrupting the current slowly. Since also, as already mentioned, the physiological effects of voltaic alternatives are very powerful, it is well to possess an instrument by which the current can at will be simply interrupted without changing its direction, or can with equal facility be interrupted and alternated. Without the aid of some instrument of this kind a galvanic battery can only be imperfectly utilised; and it is to be hoped that before long every battery intended for medical use will be supplied with a commutator as a matter of course. In certain pathological conditions, the study of the relative action of slowly and of rapidly interrupted galvanic currents possesses a high degree of interest and value, and an adaptation of Wagner's hammer (the well-known electro-magnetic "trembler") will answer this purpose.

Onimus employs a special form of interrupter, ingenious but rather complicated, by means of which, with the greatest precision, any number of interruptions from 1 to 80 in a second can be obtained.—(*Guide Prat. d'Électrothérapie*, 1877.)

Except in the therapeutics of diseases of the eye and ear, metallic rheostats (Siemens, Brenner) are superfluous for the requirements of ordinary practice, and liquid rheostats are open to serious objections.

Having now glanced at the different forms of electric currents, and the means by which they are generated and conveyed to the body, we are naturally led to inquire what is the result when the two electrodes of a galvanic battery or of an inductorium are placed upon the surface of the body; or, in other words, what are the laws of the transmission, diffusion, and localisation of electric currents in the human body. Notwithstanding the clear expositions given by several writers (Fick, Rosenthal), this subject appears still to be often misunderstood, and the most extraordinary—I might almost say superstitious—notions prevail respecting it.

Let us start with the incontrovertible proposition that the transmission of electricity through the human body is a totally different matter from the transmission of a current along a metallic wire; and for the sake of clearness we will consider for a moment the most simple case first.

1. In a linear homogeneous cylindrical conductor—*e. g.*, a telegraph wire, whose transverse section is everywhere the same, it is manifest that the intensity and density of the current in any two places will be identical; for the current may be looked on as made up of a number of parallel streams of equal intensity (Plate III., Fig. 1).

2. Let us suppose now that the conductor is divided into two branches of unequal resistance, one of which is, say, five times larger than the other, and therefore, offers $\frac{1}{5}$ the resistance. The current will divide into two sections, and the intensity will be five times greater in the larger branch than in the smaller—*i. e.*, the intensity is directly proportional to the conductivity of the conductor (Plate III., Fig. 2).

3. Similar reasoning will apply to the case of a number of heterogeneous linear conductors of equal length. The intensity of the current transmitted through each conductor will be inversely proportional to the resistance of each individual conductor.

4. If now, instead of uniform or cylindrical conductors, we pass

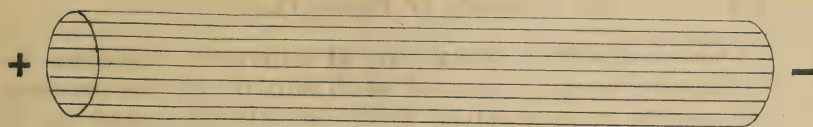


FIG. 2.

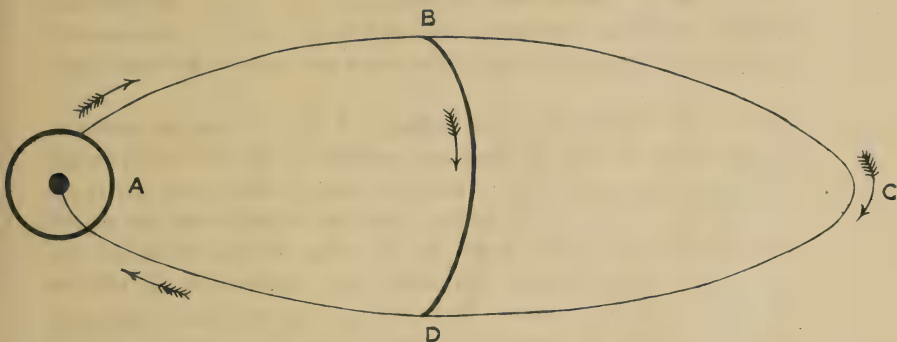


FIG. 3.

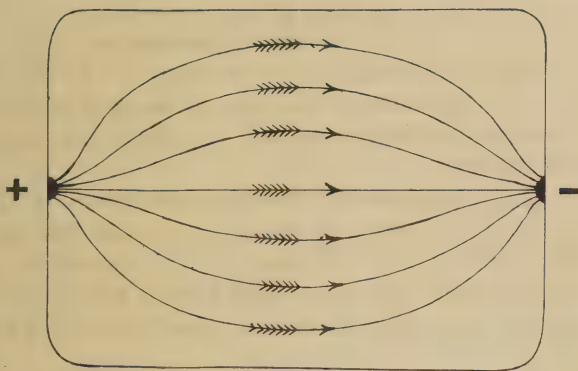
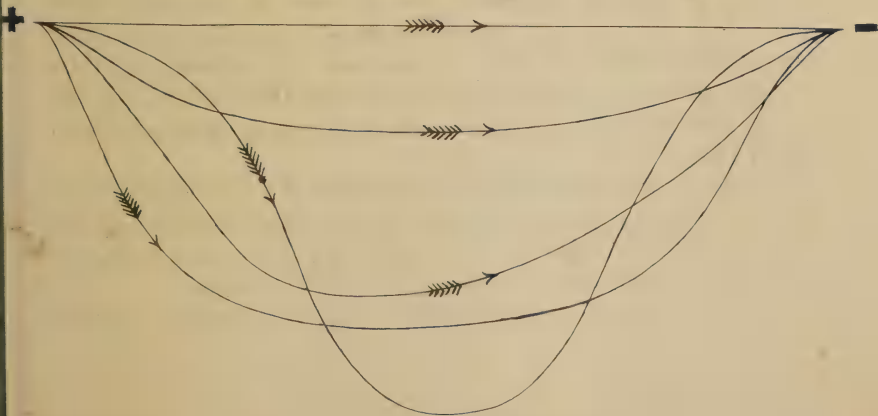


FIG. 4.



to the propagation of electricity across conductors of three dimensions, this is what will occur. Take the case of a metallic plate through which a current passes by two points diametrically opposite each other (Plate III., Fig. 3). We may, in imagination, dissect the conductor into a series of segments of equal thickness, all of which begin at the point of ingress of the current and converge again at the point of egress. The current will naturally pass through all these segments, and the conductor will be traversed by a series of curvilinear currents of different intensities, leading to and from the points of application. The current of greatest intensity will run in the right line which directly connects these two points—for it is the line of least resistance. Whatever be the form of the conductor, or the position of the points of entry and exit of the current, the relations of intensity among the ramifications of the current will remain the same, the intensity decreasing on either side of the shortest route between the two points.

5. The general course of currents in an irregular heterogeneous conductor will readily appear from what precedes, and is represented in the diagram (Plate III., Fig. 4).

It will now be clear that the diffusion of electric currents in the human body is a very complicated problem. For, in electrotherapeutics, we have, as a rule, to deal not with linear conductors, but with conductors of three dimensions, affording every variety of form, and great diversity in nature, and in specific resistance. So that it is patent that the laws of divided currents in multiform conductors have important bearings upon the medical applications of electricity.

Guided by these considerations we will be in a position to grasp the principles of localised electrification (galvanic or faradic).

Let me, in the first place, explain why, strictly speaking, *it is impossible to localise absolutely the action of electric currents in any part of the human body*—a matter not sufficiently dwelt upon.

When the poles of a galvanic battery or of an inductorium are placed upon two suitable spots on the body, the current will disperse itself from these points in all directions throughout the whole body into a multitude of more or less curved branches, which all converge at the points of application. Theoretically the entire body is filled with electric curves, and the electric equilibrium is everywhere disturbed, since no part of the multifarious conductors that constitute the body can remain absolutely unaffected so long as the current passes.

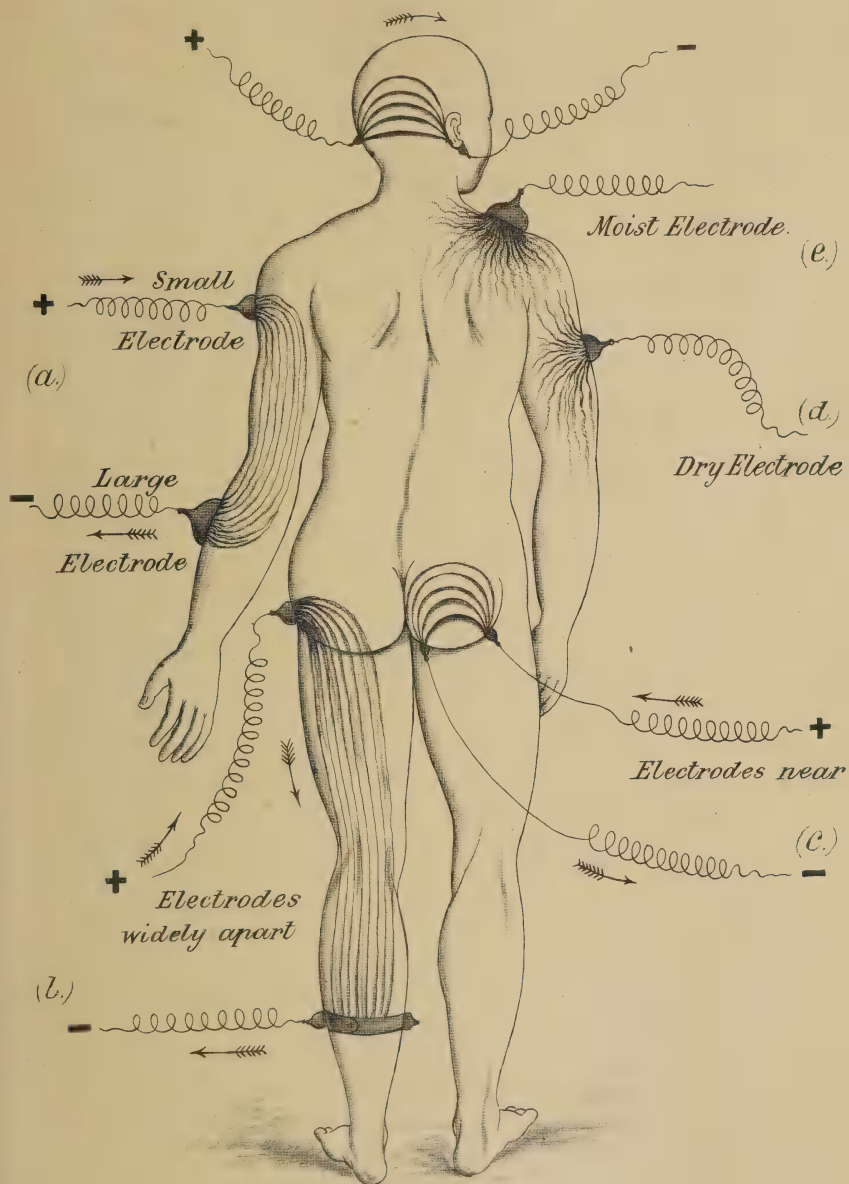
But for practical purposes we can readily manage a *relative limitation* of the current to particular parts—*i. e.*, we can arrange a current so that in a certain spot it shall have a density sufficient to produce a palpable physiological action, while in the remaining parts of the body the density of the current is too small to produce physiological effects. The density of the current is greatest at the polar points, is less along the line joining these, and will rapidly decrease with the distance from these points. The nearer the points of application the easier will be a *relative localisation* of the action.

The factors which influence the ability to cause even a relative localisation of electricity are essentially the following, most of which were empirically established by Duchenne, although he was not thoroughly familiar with the physical principles of his method:—

1. *The differences which exist in the respective conductibilities of the organic tissues.*—We do not accurately know the specific resistances of the animal tissues, but their relative resistances depend mainly on the contained saline fluids, and on their degree of concentration. It is evident that the intensity of the current will be greatest in those tissues which offer least resistance.

2. *The differences of irritability among the various animal tissues.*—If the current curves pass through tissues which happen to have a greater susceptibility to electricity than their surroundings, considerable physiological action may be exerted at points far removed from the electrodes, notwithstanding the small density of the current. A pathological phenomenon of frequent occurrence thus receives its explanation. In electrifying with a moderately strong current the *extensor* muscles of the forearm in a severe case of saturnine paralysis, it will often be observed that while the extensors refuse to act, the *flexor* muscles will respond actively. The reason simply is that the current curves passing through the inexcitable paralysed muscles reach the anterior surface of the arm, and retain there sufficient density to excite the more irritable flexor muscles. In the same way can be explained certain other phenomena that have been erroneously set down to reflex action.

3. *The possibility of altering the density of the current by modifying the dimensions of the electrodes.*—The current will be of greater density beneath the smaller electrode (Plate IV., *a*). Therefore, with one very small electrode, and one much larger, it would be quite possible that at some depth below the small electrode the density would be greater than immediately underneath the large electrode, but of course nowhere could it be greater than or



even equal to the density immediately beneath the small electrode. If, then, we are able to irritate a muscle or nerve lying at some depth it will not be possible to do so without also irritating the cutaneous nerves at least beneath the smaller electrode.

4. *The possibility of controlling the distribution of the electric curves by varying the distance which separates the two electrodes.*—Suppose the electrodes are placed *far apart* (Plate IV., *b*), say one over the nates and one on the leg, current curves of about equal length and consequent equal resistance traverse the limb, and the total quantity of electricity is evenly distributed over the cross-section of the limb. For a moderate current, therefore, the density will nowhere be competent to cause muscular contraction. Hence, also, the reason why the muscles of the trunk remain quiescent, even when the muscles of both arms are thrown into contraction by very powerful currents transmitted through electrodes grasped in the hands. Let the position of the electrodes be changed so that they both rest on the gluteus maximus *close together* (Plate IV., *c*). The intensity will now be very different in the current curves. Along the short paths much more electricity will pass than along the longer routes, as represented by the relative thickness of the lines. Therefore, immediately beneath and between the electrodes, the current will be in a state of high density, and vigorous *local* contraction of the muscle will be caused.

It is not possible to electrify one muscle lying beneath others, without the current passing through the upper muscles, yet it is possible to cause motor contraction in the inferior one alone, as is easily demonstrated on the extensor indicis muscle. [Experiment shown.] How is this effected? By placing one small electrode so that it lies directly over the “motor point” of the muscle—*i. e.*, the place of entry of the motor nerve filament into the muscle.^a These motor points, which were long ago recognised by Duchenne and Remak, and which have been so carefully illustrated by Ziemssen, are of very great practical importance, and, if time permitted, it would be easy to give numerous demonstrations of their utility as to diagnosis and treatment. The laborious researches of Duchenne on the movements of muscles, singly and associated, studied by localised faradisation, have furnished most interesting results, which have been welcomed by the biologist no less than by the anatomist.

^a The motor point sometimes represents merely the spot where the nerve, external to or within the muscle, runs sufficiently near the surface to be easily reached by electric currents.

The *rationale* of the greater excitability of muscles at these definite motor points is this:—Nerve tissue is, as is well known, more easily irritable than muscular tissue, and hence, if by means of a small electrode we direct a current of maximum density on the maximum mass of nerve fibres, we will obtain the greatest possible stimulating effect.

5. *The possibility of diminishing the resistance of the skin by employing moistened electrodes.*—The different effect of electricity according as the skin is dry or moist (in the former case, irritation of the cutaneous nerves—in the latter, of the deeper parts predominating), is, as Fick remarks, one of the earliest and most obvious empirical facts in electro-therapeutics. The physical explanation of the difference is very simple. We know that the dry cuticle of man offers colossal resistance to electricity—is, indeed, almost a non-conductor, and may be compared to an envelope of thin guttapercha, pierced by the minute apertures of the sebaceous and sudoriparous glands. Contrariwise, when thoroughly moistened, especially with warm salt solution,^a its conductivity is much increased.

(I.) Hence, during the application of two dry electrodes, especially when they are far apart, much less electricity will pass into the body on account of the immense resistance, than during the application of moist electrodes. A very small portion of the total current will penetrate into the deeper tissues—*e. g.*, into the muscles, and will nowhere possess sufficient variations in intensity to excite irritation of muscle, directly or indirectly, especially if, in consequence of a considerable separation of the electrodes from each other, the current disperses itself tolerably uniformly over the cross-section of the entire limb. How comes it, then, that in this case tingling and excitation of the cutaneous nerves are strongest at the points of application of the electrodes? The answer is this. On account of the extremely small conducting power of the dry cuticle the electricity does not issue from the electrode in a coherent stream, but rather divides itself into a number of separate rays (Plate IV., *d*), which rush in through the foramina of the sebaceous and sweat glands, the resistance along these little tubes, full of fluid, being much less. In consequence of this mode of penetration, the density of the current in each ray, taken separately, is very notable, because the cross-section of the united individual rays forms only a proportion-

^a A solution of NaCl has a minimum resistance when the solution contains about 24 per cent. of salt, or 1 part salt and 3 parts water.

ately small area. A multitude of fine currents of considerable density, therefore, impinges directly on the sensory nerves and causes irritation. But once having got beneath the skin the currents meet with better conducting media, and diffuse themselves uniformly, with corresponding loss of density, and become so weakened as to be incompetent to provoke muscular contraction. The most energetic stimulation of the skin is effected by applying one electrode, of large surface, to the moistened skin, while the other electrode is small, in the form of a metallic button, or, better still, of a wire brush, and is applied to the dry skin. From each wire of the brush (*i. e.* a multifid conductor) streams forth a current of electricity of high density, and therefore most suitable for causing irritation of the cutaneous nerves.

(II.) With two moist electrodes the results are different.

(*a.*) On account of the increased conducting power of the epidermis, as a whole, more electricity can pass into the body.

(*b.*) The current flowing from either pole now forms a coherent stream (Plate IV., *e*), because it can traverse the epidermis *en masse*, and need not seek for apertures. Once beneath the surface the current diffuses itself in all directions. Therefore, with moist electrodes, the cutaneous nerves will be less irritated, because the cross-section of the current will be plainly equal to the whole surface of contact of skin and electrodes, and accordingly the density of the current must be less than in the preceding case, in which the total cross-section was the sum of the cross-sections of the individual current rays. Of course when a mixed nerve is excited, it is impossible to avoid simultaneous causation of pain, which, however, can be lessened by pressing the electrodes very firmly on the skin (Fick, *Medicin. Physik.* 2nd Edit.).

Such are the more important principles of localised electrification, or Duchenne's method, to the applications of which anatomy and physiology, pathology and therapeutics, owe so much.

ART. VII.—*A Case of Aneurism of the Thoracic and Abdominal Portions of the Aorta.* By ROBERT SAMUELS ARCHER, A.B., M.B., Univ. Dub.; Assistant-Physician, West Derby Union Hospital, Liverpool.

The following case is interesting as bearing upon the diagnosis of intra-thoracic tumours:—

John H., aged thirty-seven years, a labourer, was admitted to West Derby Union Hospital on June 29th, 1877.

The only history of his case that I could gather from the patient was that twenty years before he came under my observation he fell off a cart and hurt his back.

About two years before admission he began first to complain of pain round the lower part of the left side of his chest, and shooting round to the small of his back on the same side. He never had any serious illness.

On admission he complained of pain and tenderness at the inferior antero-lateral region of the left side of his chest, and in the lumbar region near the spine on the same side. There was dulness on percussing the lower half of the left side of the chest, and also an almost entire absence of respiratory sounds here, which were also subdued to a considerable extent over the other portions of this side. Heart's impulse was diffused, and a systolic murmur was heard at the apex. Pulse was weak and shaky, and indicated mitral valve disease. There was epigastric pulsation. An ill-defined pulsating tumour was discovered in the left hypochondrium just projecting from under the false ribs; this could not be accurately mapped out owing to the gastric tympanites. The pulsation in the left hypochondrium was heaving and distensile. My diagnosis was abdominal aneurism, mitral regurgitation, and consolidation of lower portion of left lung. I ordered 10 grs. of potassii iod. in \mathfrak{z} i. of infus. gent. co. ter in die.

July 2.—He was reported to have passed a “bad night” from the pain in his back, and in the lower part of his left chest.

July 3.—He slept well last night, and the pain was much better.

July 6.—To-day for the first time I detected a blowing murmur over the tumour. The condition of the lower part of the left chest continues as on admission.

July 24.—Since the last report he had passed a very easy time till last night, when he had a return of the pain in his back, of a more acute character than usual. Abdomen rather distended and tympanitic, and as his bowels had not acted for two days he was ordered a dose of castor oil.

July 25.—Pain in back slightly better.

July 30.—A good deal of pain in back all night.

August 10.—He has been complaining more or less of pain in the old positions since last report; at times he has suffered intensely. Decubitus was at this time diagonal, right-sided, and frontal, his knees being semi-flexed, and thighs semiflexed on abdomen.

Sept. 27.—Still the pains have continued as before, but, in addition, during the last few days he has complained of pain in the left hip. No pain at seat of impulse in left hypochondrium.

Oct. 26.—The tumour was observed to have become much more prominent, and there was now a well-defined pulsating mass. He now complained very much of the pain in the left hip, referred to a point about mid-way between the great trochanter and the anterior superior spine of the ilium. This pain had now become of a shooting or lancinating character, and ran down the antero-external aspect of the limb to the knee.

Oct. 31.—Suffered from acute pain in the right side of the abdomen last night, but was easier when I made my morning visit.

Nov. 2.—Bowels had not been opened for five or six days, so an enema was ordered. The pain was replaced by anæsthesia in the left thigh, so that he did not feel pinches or pricks in the least in this region. There was pain over the seat of impulse and down the right side of the abdomen.

Nov. 5.—Says that “whenever he takes any food it drives a pain down his left leg.”

Nov. 10.—Pulse has become very feeble; perhaps somewhat weaker in left wrist than in right. Pain has again manifested itself in the left thigh.

Nov. 12.—To-day he complained of weakness of left arm. A distinct harsh systolic bruit was heard over the entire cardiac region, and was more feebly audible posteriorly.

Nov. 27.—There has been acute pain latterly in the back, especially at two points—one of which was situated about the last dorsal vertebra, and the other in the lumbar region, about two inches to the left of the spine.

Dec. 3.—Much worse in every respect; large mucous râles audible over the chest; tracheal râles.

Dec. 4.—Urine has become very scanty and much concentrated. Sinking. He died at 5 45 p.m. According to the nurse's account he made a violent spring forwards and attempted to get out of bed, but sank quietly back, and his sufferings were terminated.

Autopsy.—Dec. 5.—Dead about twenty-seven hours. Rigor mortis well marked; body very much emaciated; hypostatic congestion moderate.

Thorax^a.—The left lung adhered to the parietes of the chest, and fully

^a The pathological specimen was exhibited at the meeting of the Liverpool Medical Institution, Jan. 17.

its lower half was compressed against the ribs, being spread out over the surface of a large aneurismal tumour of the descending portion of the thoracic aorta. This layer of lung was about half an inch thick, condensed, of a blackish colour, and had almost lost the characteristics of pulmonary tissue. The upper half of this lung was heavy and soddened, in a condition of œdema, frothy fluid streaming out on incising it. There was some fluid in the upper portion of the left pleural cavity.

The right lung was congested and œdematous. The pleural cavity contained a considerable quantity of fluid, and there was some recent gelatinous-looking lymph effused on the diaphragmatic surface of this lung, and on the corresponding portion of the muscular expansion which separates the chest from the abdomen.

The pericardium contained some turbid greenish fluid. This serous membrane had contracted adhesions with the pleura on the left side.

The heart was nowise hypertrophied nor dilated, and appeared healthy in every respect; its valves and auriculo-ventricular openings were normal; the left perhaps slightly enlarged.

The arch of the aorta was thickened and its internal surface rugose, and exhibited traces of atheroma. At the termination of the arch a large aneurismal sac commenced, stretching thence to the diaphragm, and compressing the left lung, as described above. The aneurism filled more than half of the left side of the chest, extending in front of and encroaching beyond the right side of the spinal column. The œsophagus ran along the right anterior aspect of the sac. The heart, as it lay *in situ*, was in front of the tumour, pushed forward, and slightly to the right. The sac was occupied by a firm greyish red clot of laminated fibrin, which, at one part in the right inferior portion, seemed to have a tendency to purulent degeneration. The sac adhered intimately to the vertebræ from the third to the seventh dorsal, both inclusive, and had deeply and extensively eroded their bodies, the intervertebral substances remaining intact. The lining membrane was thick and rough.

Abdomen.—A tumour was found projecting from under the false ribs on the left side. The most prominent portion of this tumour was encircled below by the lesser curvature of the stomach; it extended down the left lumbar region to the brim of the pelvis; it lay in front of the spine, and encroached towards the right side. The left kidney adhered to the mass, and its upper part was to some extent flattened by it. The greater portion of the small intestines lay in the right inferior part of the abdominal cavity. The inferior vena cava coursed along the right side of sac, and its bifurcation lay in front of it, slightly towards its right side. The anterior wall of the tumour in its lower division was formed by the lumbar muscles and fascia of the left side. The aorta was seen running along the lower and anterior aspect of the sac for the distance of about three inches before its bifurcation, which also was

situated on its surface. On dividing the psoas muscles at the brim of the pelvis on the left side, a quantity of semi-fluid blood, of the consistence and colour of thin black currant jelly, oozed out, and my hand readily slipped into a cavity filled with this substance. I dissected up the mass of the lumbar muscles and fascia as carefully as possible, and, as I proceeded, a very large quantity of dark semi-fluid clots escaped, till at length the true sac was reached, which was about the size of a large cocoanut, and was filled with soft brownish red, indifferently laminated fibrin. The posterior wall of the true sac was formed by the lumbar vertebræ—all of which, with the exception of the last, were deeply eroded, if possible more so than their fellows in the dorsal region. The right margin of the sac was limited by the right psoas muscles. There was a secondary dilatation of that portion of the true sac from which the celiac axis sprung, in fact involving this vessel, and from this its branches arose. This secondary sac was about the size of a large plum, and it was it that formed the most prominent portion of the tumour observed during life.

The measurements of the sacs were as follows:—The thoracic tumour was 6 inches long, commencing $1\frac{1}{2}$ inches below the orifice of left subclavian artery. The clot contained in this sac was 6 inches in length, and the circumference of its short axis was 10 inches. The length of the portion of aorta between both sacs was 2 inches. The entire abdominal tumour was between 8 and 9 inches in length; the true sac about $4\frac{1}{2}$ inches.

The kidneys were slightly congested. The spleen was softer than normal, and there was a small cretaceous nodule in its upper part. The other abdominal viscera exhibited no manifest traces of disease.

The existence of two aneurisms of such enormous dimensions, involving the thoracic and abdominal portions of the aorta in the same subject, is a circumstance of no common occurrence, and one which rendered the diagnosis of the chest lesion in this case more difficult than it possibly might otherwise have been.

My much-beloved, revered, and famous teacher, the late Dr. Stokes, speaking generally of thoracic aneurism, remarks:—"Aneurism of the thoracic aorta does not present any special symptoms, and occasionally may be one of the most latent of diseases; its symptoms, such as they are, are anything but constant; and this we should expect when the varied sources of these phenomena are considered."^a One can easily imagine how, in a case such as the present, with a well-marked and undoubted lesion of the abdominal aorta, the attention of the observer was concentrated,

^a Diseases of the Heart and Aorta. Page 558.

as it were, on the latter, which indeed accounted for the majority of the symptoms; but, at the same time, in the absence of this, I doubt if we should have come to a correct conclusion concerning the thoracic tumour. Notwithstanding the testimony of Stokes regarding the rarity of cardiac disease existing with abdominal aneurism,^a I considered that H.'s case was an exception to the rule observed by this distinguished physician. The apical systolic murmur, the feeble, shaky, uncertain pulse, and the apparent cardiac hypertrophy, indicated by increased area of præcordial dullness—all, to my mind, were symptoms of mitral valve disease; so characteristic was the pulse that, after having felt it, I should have been disappointed had I not heard a bruit at the apex. Even at the last, when the murmur had extended over the entire cardiac region, and was audible feebly behind, I did not see my way to alter the opinion erroneously arrived at in the first instance, so strong is the force of first impressions. Although this patient was examined by several physicians, it flashed across the mind of none of them to assign to their true cause the dullness of the lower half of the left chest, the systolic murmur, the diffused impulse, and the apparent cardiac hypertrophy, but they all, in the main, agreed with the interpretation I put on the origin of these phenomena. Niemeyer observes that “as long as the aneurism remains enclosed within the thorax, without touching its wall, diagnosis is not assisted by physical examination.”^b When we consider the peculiar anatomical relations of the sac, it will be granted that obstacles of no ordinary kind stood in the way of a correct diagnosis. In the first place, there were the more or less unyielding and rigid thoracic parietes preventing the detection of abnormal pulsation by manipulation—a means which is so useful in discovering abdominal tumours. Secondly, the intervention of a layer of lung tissue prevented, by its elastic resistance, any shock which might otherwise have been communicated to the chest wall, from being perceived. Thirdly, the absence of pressure symptoms, which are so valuable in the diagnosis of intra-thoracic tumours, was a circumstance by no means calculated to throw light on the obscurity of the present case. The slight feebleness of the left pulse, as compared with the right, might have been some assistance, but no importance was attached to the trivial, almost imperceptible difference, as it is common enough to observe this without a cause. In the absence of

^a Diseases of the Heart and Aorta. Page 615.

^b Text-book of Practical Medicine. Vol. I., p. 407.

post mortem evidence of dilatation of the left auriculo-ventricular orifice, there appears to be three possible sources from which the systolic apical murmur may have proceeded:—(1). It might have been hæmic. (2). The ventricular systole in a confined pericardial sac, narrowed in its antero-posterior diameter, might have caused it. (3). The most probable origin of it was in the sac itself. It was not the first, because hæmic murmurs, almost without exception, are heard at the base of the heart. From its character, it is by no means probable it was produced by the action of the heart in a contracted pericardium, so the source of it must have been in the sac itself. The fact of its being propagated over the entire cardiac region, and being heard feebly behind towards the termination of the case, is a strong argument in favour of its aneurismal origin. Dr. Sibson^a tabulates forty-six cases of aneurism of the descending thoracic aorta, with the following results:—Erosion of vertebræ occurred in 74 per cent.; pulsating tumour, 26 per cent.; murmur, 4·3 per cent.; dyspnœa, 26 per cent.; orthopnœa, 4·3 per cent.; cough, 24 per cent.; hæmoptysis, 19·5 per cent.; dysphagia, 8·7 per cent.; pain, 67·3 per cent; so that, according to this analysis, murmur appears to be a rare symptom. This case exemplifies very well the following remark of Dr. Stokes concerning the existence of aneurism without manifest physical signs:—"To those whose knowledge of aneurism is derived from written descriptions rather than bed-side experience, it may appear strange that a disease of such importance should ever exist without being accompanied by signs which, if not sufficient to determine the nature of the affection, would at all events indicate some important organic disease. Yet cases do occur in which, at some period at least, the disease may elude the most careful investigation; and proof is not wanting of this latency even after a copious hæmorrhage has taken place from the sac itself."^b All authorities agree pretty generally as to the impossibility of diagnosing accurately many of these tumours.

At a recent meeting of the Medical Institute of this town, my friend, Dr. Pollard, detailed a case in which death occurred eleven days after the rupture of an aneurismal sac of the descending aorta into the left pleura. The patient had consulted numerous physicians in London and elsewhere for undefined pains in the limbs, &c. So obscure were the symptoms that attention was not

^a Medical Anatomy.

^b Diseases of the Heart and Aorta. Page 608.

directed to the chest till dyspnœa and other signs of effusion into the pleura appeared; then empyema was diagnosed, but on the introduction of an aspirator needle only a small quantity of blood escaped.

In the Transactions of the London Pathological Society^a there is reported a case of aneurism of the descending aorta, causing death by rupture into the œsophagus. The subject of this case was a soldier, aged thirty-four years, in whom the most prominent symptoms were occasional pain in the back and vomiting. He performed his military duties almost up to the time of his death. Aneurism was not suspected during life, but at the autopsy a sac was discovered lying in front of the spine, from the third to the sixth dorsal vertebræ, and there was erosion of this portion of the back bone.

Dr. Walshe observes, concerning the diagnostic significance of murmurs, that they “must be stronger over the sac than over the heart to have any diagnostic value.”^b One of the affections which this author mentions the possibility of confounding an aneurism behind the heart with is hypertrophy. These are his words: “The affection with which an aneurism thus seated may be most readily confounded is hypertrophy of the heart; the strong systolic impulse and the diastolic impulse combine to deceive.”^c This was part of the error fallen into in the present case.

Whilst the subject of these remarks was under observation, there lay in the bed next to his a man whose case I studied in connexion with this one, and in whom, in the earlier stages, many of the symptoms strongly resembled those of abdominal aneurism. He was fifty years old. There was a pulsating tumour in the epigastrium; more or less severe abdominal pain; pain in the back and over the seat of impulse, where there was also a murmur; pains under the false ribs on left side, but there was an absence of the shooting pains down either lower extremity; there was also history of an injury to the back. Early and rapid emaciation set in, and his face assumed a ghastly look, and that peculiar dingy “malignant” tinge—so, at the suggestion of a medical friend, the diagnosis of aneurism was changed to that of a malignant tumour lying in front of the aorta. At the *post mortem* examination a

^a Case reported by Mr. Long for Mr. Trotter, Dec. 1st, 1863. Vol. XV., p. 73.

^b A Practical Treatise on Diseases of the Heart, Lungs, and Aorta. P. 768. 1854.

^c Loc. cit. Page 769.

cancerous mass was found almost surrounding the aorta reaching from above the coeliac axis to within a short distance of its bifurcation. Here was a case in which a series of symptoms, pointing strongly to abdominal aneurism, existed—in fact, one might say they were pathognomonic of this affection, but how different was the true condition of affairs.

The temporary onset of anæsthesia in the left thigh, and limited to this region for a day or so, about a month before the patient's death, is a phenomenon for which I cannot give any correct explanation, but suppose it depended on some transitory interference with the conduction of sensory impressions, resulting from pressure on the nerve fibres, either by the sac or a loaded condition of the rectum. Whatever may have been the cause of it, it was of a very temporary nature, as after a short interval the unfortunate patient was attacked with the old pain, and if possible more severely than before.

I am inclined to regard the formation of the false sac beneath the psoas muscles as probably of tolerably recent date—I should say some time during the five months he was under observation, and I think not unlikely during the last month or two of life. The clots were so fluid, and appeared so like recently effused blood—there being no deposit of fibrin, or other circumstance to indicate an old effusion—I cannot believe this portion of the tumour to have existed for long; nor do I think it occurred suddenly, for there was no constitutional symptom which would point to so sudden a shock to the vascular system as such an occurrence would be likely to produce. I think the formation of this secondary sac must have been a more or less gradual process. The true sac, doubtless, insinuated itself beneath the muscular expansion; after a time rupture occurred, and blood was extravasated, and this by comparatively slow steps, but none the less sure, little by little burrowed its way on till at length the sac assumed the dimensions discovered after death; and I think it quite possible, had the patient lived for a sufficiently long time, that this boring process might have gone on to such an extent as to form a fluctuating tumour, pointing below Poupart's ligament.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Ziemssen's Cyclopædia. Vols. XII., XIV., XV., XVI. London: Sampson Low and Co. 1877, 1878. 8vo.

THESE are the four latest volumes of the *Cyclopædia of Practical Medicine* edited by Dr. H. von Ziemssen. The volumes, although not issued consecutively, yet make their appearance with a regularity which is highly commendable, and which contrasts favourably with the advent of some kindred English works on Practice of Medicine. Vol. XII. is devoted to Diseases of the Brain and its Membranes; and we notice with gratification that the subject of Tumours of the Brain and its Membranes by Obernier has been ably translated by Mr. H. R. Swanzy of this city. The section on Syphilis of the Brain and Nervous System is well worth attentive perusal, for at present it seems that syphilis of the nervous system, as being comparatively the darkest domain of internal syphilis, is precisely the point which, within the last few years, has most attracted the attention of physicians. The subject of General Paralysis of the Insane is discussed at length by Hitzig in the section on Hypertrophy and Atrophy of the Brain, inasmuch as marked atrophic conditions of the cerebellum are very frequently present, and indeed always exist in protracted cases of this disorder.

Vol. XIV. of the *Cyclopædia* is devoted to Diseases of the Nervous System and Disturbances of Speech; while Vol. XV. deals with Diseases of the Kidneys, and forms a very respectable monograph on the subject. The subject of the movable (migratory) kidney, its history and literature, etiology, pathology, symptomatology, and diagnosis, is discussed in full detail, and will prove instructive to those who are sceptical on the reality of the condition.

Vol. XVI. treats of Diseases of the Locomotive Apparatus, and General Anomalies of Nutrition. Under the former heading are discussed rheumatism and gout; under the latter the interesting subjects of anæmia, chlorosis, corpulence, scrofulosis and affections of the lymphatic glands in general, diabetes mellitus and insipidus.

Along with Vol. XVI. the publishers, Messrs. Sampson Low and Co., have issued a notice to the subscribers to this work,

calling their attention to the fact that in the early announcements subscribers were promised fifteen volumes of from 500 to 700 pages each. Very soon, however, after the translation had been commenced, it became apparent that the German authors were not restricting themselves closely to the proposed limits, and that the volumes were not only very much larger, but supplementary ones even were being added to the series. A proposition to abridge or condense in this translation met with very general objection. The publishers have, therefore, done their utmost in endeavouring, while giving every line of the original, to confine the work within the advertised limits. At wholly unexpected and very largely increased cost, volume after volume of nearly 1,000 pages has been published; but, notwithstanding this fact, the publishers still find themselves obliged very reluctantly to extend the work to seventeen volumes. The work in the German, with the exception of the volume on Skin Diseases, by Prof. Rindfleisch and von Ziemssen, is practically finished, so that it is absolutely certain that seventeen volumes will complete the work.

We cannot help thinking that each of the subscribers will heartily approve of this action on the part of the publishers, which will have the effect of furnishing them with a Cyclopædia of Practical Medicine worthy of the name, and at a cost which is but nominal in comparison with the value of the work.

Nathan the Wise; a Dramatic Poem. By LESSING. Translated into English blank verse by ANDREW WOOD, M.D., &c., &c.

DR. WOOD, whose translation of Schiller's "Don Carlos" we reviewed some time since, has again amused his leisure with a translation from the German, this time entering the lists with the already numerous translators of Lessing's "Nathan der Weise." In choosing this play he has set himself a more difficult task than the last. Not merely is the language of Lessing more difficult to render into English than that of Schiller, so transparent in its classical simplicity, but the whole charm of Lessing's play consists in the delicate humour with which he points his obvious moral, religious toleration, and the artistic variations of style by which he gives a semblance of life to his characters, which are essentially little better than bloodless puppets. This is just what is likely to evaporate in a translation; and in Dr. Wood's rigid word for word rendering of Lessing's German it does evaporate most completely.

The characters of Saladin and Al Hafi, preposterous in the original, become shapeless and unintelligible in the translation; the fiery Templar, with his German, "sturm und drang," expresses his passion in the colourless and humdrum style of the other personages; even the Lay Brother loses that humorous mixture of shrewdness and simplicity which makes him more of a real character than the other *dramatis personæ*. Perhaps, however, it is too much to expect a translator to preserve this subtle essence of his author's style; but, alas! what becomes of the author when this has escaped? What becomes in especial of poor Lessing, who has no valuable substance, whether in the shape of plot, dramatic situation, or wealth of thought, to fall back upon when once stripped of his style? An impromptu of Chopin, played by a school-girl to the merciless ticking of the metronome, in thus losing its aroma, loses everything; and so does "Nathan der Weise" in Dr. Wood's mechanical verse. It is not a great work even in the original—it is nothing at all in the translation.

Dr. Wood, in his preface, challenges comparison with the existing version by William Taylor, of Norwich; and to do him justice, we must admit that he generally gives the verbal sense of the original more accurately than Taylor. On the other hand, Taylor's verse, without being by any means good verse, is less mechanical than Dr. Wood's, showing, as it does, some little sense of rhythm.

To sum up, Dr. Wood has given us, on the whole, a painstaking and tolerably accurate translation of Lessing's *words*. He might, perhaps, even have given us a tolerable translation of the *play* if he had written it in honest prose; the characters would probably have lost less in colour if presented through some medium less foreign to the translator's genius than verse. As it is, his conscientious desire for verbal accuracy has helped to betray him into that rigid and lifeless style which is his main fault.

As a specimen of Dr. Wood's work, we give a portion of the scene between the Templar and the Lay Brother, in the original and in his translation:—

Tempelherr. Habt ihr nicht gehört? nur erst gehört
Was für Verbindlichkeit dem Saladin
Ich habe?

Klosterbruder. Wohl hab' ich's gehört.

Temp. Und doch?

Klost. Ja—meint der Patriarch,—das wär' schon gut:
Gott aber und der Orden . . .

Temp. Aendern nichts!
Gebieten mir kein Bubenstück!

Klost. Gewiss nicht!
Nur—meint der Patriarch,—sei Bubenstück
Vor Menschen nicht auch Bubenstück vor Gott.

Temp. Ich wär' dem Saladin mein Leben schuldig:
Und raubt' ihm seines?

Klost. Pfui!—Doch bliebe,—meint
Der Patriarch—noch immer Saladin
Ein Feind der Christenheit, der Euer Freund
Zu sein, kein Recht erwerben könne.

Temp. Freund?
An dem ich blos nicht will zum Schurken werden,
Zum undankbaren Schurken?

Klost. Allerdings!—
Zwar—meint der Patriarch,—des Dankes sei
Man quitt, vor Gott und Menschen quitt, wenn uns
Der Dienst um unsertwillen nicht geschehen.
Und da verlauten wolle,—meint der Patriarch,—
Dass Euch nur darum Saladin begnadet,
Weil ihm in Eurer Mien', in Euerm Wesen,
So was von seinem Bruder eingeleuchtet.
&c., &c.

Templar. Have you not heard,
Or do you now first hear what obligation
To Saladin I'm under?

Lay Brother. Yes, I've heard.

Temp. And yet?

Lay Brother. Yes—thinks the Patriarch—that is well;
But yet God and the Order. . . .

Temp. Matter not!—
Impose on me no villainy!

Lay-B. O no!
Only the Patriarch thinks that villainy
'Fore men is not, too, villainy 'fore God.

Temp. To Saladin I owed my life; and yet
Robb'd him of his.

Lay B. O fie!—yet Saladin—
The Patriarch thinks—still aye remained a foe
Of Christendom, and can acquire no right
To be your friend.

Temp. A friend?—'gainst whom I'll not
A knave, and an ungrateful knave, become.

Lay B. O, certainly! In truth—the Patriarch thinks—
That we are quit of thanks 'fore God and man,
If for ourselves the service was not done.
'Tis rumour'd—so the Patriarch says—that you
From Saladin, on this account, found favour,
Because he, in your mien and character,
A striking likeness of his brother found. . . .

&c., &c.

It will be perceived that, even in point of literal accuracy, Dr. Wood is by no means absolutely faultless. “Do you now first hear” can scarcely be accepted as an accurate translation of the words “Habt ihr nicht nur erst gehört.” “Have you not heard—only this moment heard,” would surely be nearer the literal sense of the line—*erst* meaning *just*, not *first*. And, lower down, “doch bliebe Saladin ein Feind der Christenheit,” is badly rendered by “Saladin still aye remained a foe of Christendom.” The delicate suggestion of the Patriarch’s insinuations, conveyed by the Lay-Brother’s subjunctives, will scarcely bear translation, but Dr. Wood might have put the sentence in a conditional form.

Hydatids of the Lung. By S. DOUGAN BIRD, M.D.; L.R.C.P., &c.; Lecturer on Materia Medica and Therapeutics in the University of Melbourne. Second Edition. Pp. 62. Melbourne, Sydney, and Adelaide: Robertson. 1877.

DR. BIRD has produced an excellent little monograph on a disease of which we know very little in Europe. It is only a *brochure*, but it has the especial recommendation of being the only work on hydatids in which the rules for diagnosing this disease in its early stages in the lung are clearly laid down. European authorities have either ignored the lesion altogether, or else they merely state that the presence of echinococci in the sputa will reveal its existence. It must be admitted, however, that in these countries it is exceedingly rare. Andral records five cases, Trousseau two, and Laennec only one. At the Antipodes, however, there seems to be no lack of material for observation. Dr. Bird has himself seen more than one hundred and fifty cases, and he mentions other Australian physicians as having many instances of it in their practice. He believes the dried and pulverised droppings of sheep dogs, containing ova, to be the chief means of spreading the disease, and that in Melbourne the use of the unfiltered Yan Yean water,

coming as it does from innumerable creeks and sheep runs, brings in the contagion to the city. The symptoms are—dyspnoea, duskiness of skin, a phthisical-looking cachexia, with progressive loss of flesh, clubbing of the finger ends, and incurvation of the nails, cough, and mucous expectoration, occasionally tinged with blood. These symptoms may all be present even without the complication of tubercle. The following are the physical signs observable in the case of a cyst which has progressed to the capacity of a pint or more of fluid without interference from within or without—expansion more or less deficient on the affected side; mensuration but little affected; absolute dulness on percussion, with absence of respiratory sounds over a space of the chest wall not smaller than the palm of the hand, generally in the lateral or infra-clavicular regions, and absence of vocal fremitus in most cases. This dull space always presents a rounded outline, is limited by a line of demarcation so exact that it can be mapped out with pen and ink, and is unaltered by position. Beyond the boundary line percussion is clear and normal. The respiratory sounds, though inaudible over the dull surface, commence immediately beyond the pen line, and though probably rather harsh and puerile in character, are indicative of healthy lung tissue. A localised pleuritic effusion confined by adhesion would answer to the above physical signs, but such would be very rare, and it would probably be preceded by a history of pain and febrile symptoms. The diagnosis can thus, the author thinks, be reduced to a certainty. As to treatment, the only remedy is to tap the cyst through the chest wall by means of a trocar at least six inches in length, and of the smallest diameter that is made, always taking care that it is strong enough to bear the strain of a firm pressure. The point of the stilet must be very sharp, so as to pierce at once through the adhesion of the pleura or a cyst unusually tough. If from a cyst calculated to hold a pint only an ounce or two of fluid flows, the probability is that there are unbroken daughter-cysts which must be penetrated by withdrawing the stilet a little and making fresh punctures. Of course the neighbourhood of large vessels is to be avoided. Should any of the larger bronchi be pierced the rush of fluid into the air-passages will be for some hours alarming, but no fatal case has been observed. On the contrary, Dr. Bird has seen no untoward consequences resulting from tapping, so that he unhesitatingly urges it as soon as a cyst is diagnosed, especially in individuals disposed to tubercular deposit. Aspiration he does not think advisable, nor the injection of any

astrigent after the withdrawal of the fluid; but inhalations of turpentine and kamela may complete the destruction of the parasite after the operation; before it they are useless. In exceptional cases of old standing, when there is a thick adventitious external wall to the cyst, which is generally closely adherent to the ribs, or in cysts of the pleura, a free incision should be made. Dr. Bird writes plainly and concisely, and his little monograph cannot fail to take its position as an authority on the subject of which it treats.

Transactions of the College of Physicians of Philadelphia. Third volume. Pp. 214 Philadelphia. 1877.

THE "Transactions" are a series of thirteen papers, mostly on surgical topics, and of a very practical nature. One of the most exhaustive relates to the discoloration of skin caused by nitrate of silver internally. Dr. Pepper discusses (1) the cause of the discoloration; (2) the shortest period of administration, and the smallest quantity of nitrate of silver after which it has been known to occur; and (3) the best mode of avoiding this unfortunate result. With regard to the first of these points the fact that the discoloration is not limited to the external surface, but affects also the internal organs, goes far to prove that it is not due to chloride of silver, but to a deposit of the metal or its oxide, and this view is confirmed by chemical examination. With regard to the second, the smallest amount which has ever been known to produce argyria was 120 grains, given in the dose of half a grain daily for eight months consecutively; and there is no case of discoloration on record in which the use of the medicine had not been continued beyond three months. Thirdly, Dr. Pepper's rule for administration is to give it in doses of from one-fifth to one-third of a grain thrice daily, until thirty grains have been taken; then to suspend it for one month; after that interval to resume and continue its administration until fifteen grains more have been taken; after another month's suspension to resume it till sixty grains have been reached. The most valuable part of the paper, however, is the observation of a blue line on the gums as the earliest sign of argyria occurring before there is any danger of the slightest skin discoloration. Dr. G. B. Wood had certainly made mention of this sign, but it seemed to have been forgotten till the writer of this paper had discovered it for himself and called attention to it. He describes it as being very like the blue line of lead poisoning, except that it

is a little darker and more decidedly blue, and he also states that there is a tendency for the discoloration to extend to a greater distance on the gum. There is an interesting paper on the "*Post-mortem Imbibition of Poisons*," summarising results obtained by burying animals in whose stomachs poisonous solutions had been placed after death, and after the lapse of some weeks searching for the poison in other viscera. The results are well calculated to throw open a new field of doubt, in which the prisoner's counsel will have ample scope. We alluded to this subject in our *Report on Medical Jurisprudence* in the February number of the *Journal* (page 143).

- 1 *A New System of Medicine, entitled Recognisant Medicine; or the State of the Sick.* By BHOLANOTH BOSE, M.D., Lond. Pp. 212.
- 2 *Principles of Rational Therapeutics.* By the same Author. Pp. 88. London: J. & A. Churchill.

THESE books have been brought out at the same time, and with great propriety, as anyone who fancies he is able to understand the first will find the second an indispensable auxiliary. From a writer who holds the degree of M.D. from the University of London one has a right to expect something neither commonplace nor valueless. The first requirement has been amply satisfied. Dr. Bose's works are not only strikingly original, but they even suggest the idea that his examiners overlooked in him a rare specimen of psychological idiosyncrasy. Discontented with the undeniable fact that medicine is not yet an exact science, the author resolves to place it on the same basis as mathematics. This is to be effected by the rather paradoxical plan of discarding symptoms and physical signs as indicative of disease, and substituting the "state of the sick" as the only ground of a diagnosis. In other words, a general impression, which may be gained by inspiration or otherwise, is the unfailing guide to scientific exactness. The keystone of the system is the distinction drawn between *cognisance* and *recognisance*. An ulcer is a cognisant disease, but the cachexia induced in the body suffering from ulcers is recognisant. The germ of truth at the bottom of all this transcendentalism is the tendency amongst half-educated practitioners to treat symptoms rather than the disease from which the symptoms spring. It might have been thought that the therapeutics for such a general ultimate idea would merely amount to directions about obviating the tendency to death. On

the contrary, they seem somewhat complex. There are bitubulo-cell medicines, tritubulo-cell alteratives, and sentio-tonics, among the last of which are enumerated affectionate emotions and "gentle pressure." It is claimed that the new system of therapeutics is a field in which allopath, homœopath, and specialist may all find their differences reconciled, and may unite in a happy family. The claim may be a just one if all those entrusted with the care of the sick would be content to submerge all clearness of idea beneath a mass of unintelligible jargon, which has as much resemblance to a philosophical treatise as "Alice in Wonderland" has to the "Year Book of Facts." We cannot see how the Messrs. Churchill will serve either themselves or the medical profession in publishing such works.

A Course of Operative Surgery. By CHRISTOPHER HEATH, F.R.C.S. London: J. & A. Churchill, New Burlington-street.

PART V. brings this excellent publication to a close. Mr. Heath has performed the task which he set himself carefully and well. In twenty plates he has given illustrations of the principal operations in surgery, the various sections being first made on the *cadaver* and then copied by M. Leveillé. The explanatory text is sufficiently full. Altogether it is a most useful and necessary contribution to professional literature.

Illustrations of Clinical Surgery. By JONATHAN HUTCHINSON, F.R.C.S. Part IX. London: J. & A. Churchill, New Burlington-street.

WE have in this fasciculus a continuation of the subject of injuries to the head—the plates illustrating some forms of fracture, and the state of the eyes after paralysis from laceration of the cervical sympathetic. One of the inaccuracies which is frequently repeated respecting the effect of vaso-motor paralysis on the pupil is that it causes contraction. In a case given here it is demonstrated that it only prevents dilatation. When in full light the pupil of the uninjured side is moderately contracted, then there is no difference, and a difference only becomes apparent when by shading the eyes the uninjured one is allowed to dilate. The symptom is often overlooked, because it is not properly sought for. The two symptoms which, according to Mr. Hutchinson, denote vaso-motor

paralysis in the cervical region, are a diminished palpebral aperture and the inability of the pupil to dilate when shaded. Notes of several cases are given, and upon these the author makes most valuable comment. The work grows in value.

Transactions of the Medical Society of the State of Pennsylvania.
Vol. XI. Philadelphia: Collins. 1877.

THE Report of the "Transactions of the Medical Society of the State of Pennsylvania," at its twenty-eighth annual session, is a large volume of nearly 800 pages. It would be impossible to review so large a book in detail, but suffice it to say that it contains much that is interesting and even astonishing. Dr. H. Lennox Hodge, in his Address in Surgery, mentions a case of a supposed ovarian tumour, in which ovariectomy was undertaken, where the disease was found to be a renal cyst, which was removed, together with the kidney, and the patient recovered. We do not think that anything like this case has come within the personal experience of ovariectomists in this country, and as there may be many other equally strange things recorded in this volume which have escaped our notice, we recommend the Transactions of this Society to those who have leisure to read them.

Diseases of the Kidney, and Urinary Derangements. By W. HOWSHIP DICKINSON, M.D., Cantab. In Three Parts. Part II.—Albuminuria. London: Longmans, Green and Co. 1877. Pp. 400.

THE author proposes to issue this work—which is to treat of all the more important disorders which either belong to the kidneys or are mainly declared by the urine—in three parts, to form ultimately one volume. The first instalment, which was on the subject of Diabetes, has been already favourably noticed in this Journal. The present volume is a second edition, brought up to the level of existing information, of a treatise on Albuminuria, which has been long out of print. The work is illustrated with eleven plates and thirty-one woodcuts, which renders its price—twenty shillings—moderate for any who desire a modern and authoritative work on all which is included in the belongings of "Albuminuria."

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

SAMUEL GORDON, M.D., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, February 6th, 1878.

DR. THOS. FITZPATRICK, in the Chair.

The late Drs. Stokes and Churchill.

DR. DUFFEY reported that since the last meeting of the Society they had lost by death Dr. William Stokes, one of the Vice-Presidents of the Society, and also one of its best-known members, Dr. Fleetwood Churchill.

DR. HENRY KENNEDY said it became his duty to move a resolution in reference to the deaths of those distinguished and lamented members of the profession. He need not say anything in reference to the character of Dr. Stokes. He was universally known, and was regretted by all, and was perhaps the greatest loss that the profession in Ireland had sustained during the last quarter of a century. He was one of the first Vice-Presidents of the Society, and always steadily attended its meetings as long as he was able to do so. Dr. Churchill, too, occupied a foremost place in the profession, and though not latterly mixing so much as he used among his brethren, continued to take an active interest in the proceedings of the Society. The resolution he (Dr. Kennedy) moved was as follows:—"That at this, the first meeting of the Medical Society of the College of Physicians held since the lamented deaths of Dr. Stokes, a Vice-President of the Society, and of Dr. Churchill, the Society desire to express their sense of the irreparable loss occasioned by the removal of two such distinguished members of the profession—one the leader of Irish medicine, and the other amongst the foremost cultivators of obstetric science—and that the Honorary Secretary be requested to convey a message of condolence to the relatives of the deceased."

DR. MACSWINEY said he had the honour of seconding the resolution proposed by Dr. Kennedy. He entirely concurred in the propriety of its

being passed by the association. They would be neglectful of what they owed to themselves, and of the position of Irish medicine, if they failed in the duty of expressing what they felt at the loss they had sustained by the death of those two distinguished members of their profession, and also condolence with their families. Dr. Stokes's name would be for ever associated with the Irish School of Medicine. As long as the English language should be spoken the name of Dr. Stokes as the great ornament of Irish medicine in this century would be recorded. As to Dr. Churchill he (Dr. MacSwiney) spoke of him from personal knowledge of a quarter of a century, and a more kindly, good-natured friend, and able medical colleague, it would be impossible to meet. All who knew him as students, and subsequently became associated with him in practice in the city, acknowledged that he was ever ready to give his services in the kindest, friendliest, and most unselfish manner to junior practitioners, whenever they were in trouble or difficulty and required his advice. His kindness, intelligence, and accomplishments appeared in every department of life in which he took part—professional, public, and private.

CHAIRMAN.—As I am the oldest pupil of Dr. Stokes in the room, and as, if the number of members present were doubled, I should still not be deprived of that privilege, it is not inappropriate that I should make a few remarks upon the resolution. It is now forty-five years ago since I entered the Meath Hospital under Dr. Stokes and Dr. Graves. The year in which I entered was remarkable in this respect that in consequence of the difficulty of obtaining subjects for dissection in England we had then in the Meath Hospital a most numerous class of English students; perhaps there has not been so large a class in that hospital since. The result was that we Irishmen were stimulated to work in a way that Dr. Stokes told me he had never seen equalled. We were stimulated first by the establishment of premiums, and next by the energy of the Englishmen. They worked like men, and they forced us to work, and the result was that there never was such a session of genuine labour in the Meath Hospital as that one of 1830–1831. I would not add to what has been already said in this room, and what the periodicals both of this and of foreign countries have said in relation to the character of Dr. Stokes. As Dr. MacSwiney has truly observed, his name will survive as long as the English language is spoken. He was perhaps, without exception, the most energetic in his efforts of all those who strove to forward the school of physic of Ireland. With regard to my friend Dr. Churchill, who was, perhaps, more a contemporary of mine than Dr. Stokes, I can only say that a more kindly, good-natured practitioner than he was I never met.

The resolution was then put from the chair and carried unanimously.

Case of Obstructed Circulation. By J. P. DOYLE, L.R.C.S.I. & L.K.Q.C.P.I.

MR. PRESIDENT AND GENTLEMEN,—I bring forward this case in order to consult with you as to its probable nature, as I never saw a similar one before. The notes of it were taken by me at the bed-side, and I trust to lay before you an unvarnished statement of the facts.

CASE.—Female, aged thirty-two years, belonging to middle social position, five feet tall, slight make, dark complexion, partially anæmic and sallow, muscles soft, integuments flabby, of nervous temperament. When girl she was occasionally obliged to jump out of bed from a sensation of a choking. For many years she has suffered from palpitation, and for some months she had a leucorrhœal discharge, which ceased a year since.

She was the only member of her mother's family that was nursed by her, and who was, as I am informed, asthmatic, but lived to sixty-five years of age. None of her brothers or sisters suffered from any respiratory difficulty.

My patient resides in a damp locality in the vicinity of this city. She had no family by her first marriage.

On September 14, 1877, in conjunction with my father, I delivered her of a boy and girl, the former with the forceps, the latter by version. She made a good recovery; but, on the 10th or 11th day, I have been informed, some boils were developed on the lower extremities, which disappeared in a few days. She did not nurse. While pregnant she never enjoyed better health; but from the termination of it until the beginning of her present illness she did not feel quite herself, and the nervousness and palpitation began to return.

Between 11 and 12 o'clock on the night of October 17th, 1877, she felt a feeling of oppression about the heart and palpitation, which went away in a short time. On the following night it recurred, and her friends, thinking it one of her nervous attacks, humoured her by applying a linseed poultice, which was nearly cold. At this time she was not having her usual amount of sleep, and she had great anxiety about one of her children which was seriously ill, and on one or two occasions she went through the house partially dressed.

In the early morning of October 19th her breathing became so oppressive that she had to remain walking about, and did so until she fell in a weakness. At 10 a.m. she was seen by my father, and appeared to be dying from asphyxia. He prescribed the following mixture, an ounce to be taken every hour until breathing was relieved:

R.—Ammoniæ carbonat., gr. 40.

Liq. Hoffman, ʒii.

Inf. senegæ, ad. ʒviii.

Spt. chloroform., ʒi.

Tr. scillæ, ʒii.

M.—ft. mistura.

He also ordered a little warm whiskey and water every third hour, mustard cataplasm externally, and warmth to extremities.

At a quarter to 5 p.m., I visited her, and was informed by her friends that she appeared a little better. She was sitting up in bed, gasping for breath, and spoke in a sighing, interrupted manner. The intellect was clear, the complexion livid, and the expression one of anxiety and fear; the surface of the body and extremities felt cold and clammy, and the forehead was covered with large drops of perspiration. Temperature in axilla, 97°; bowels, which are naturally regular, are costive; tongue, clean; has a slight, dry cough, with little expectoration. Pulse, scarcely perceptible, 140; circumference of thorax, 31 inches: on inspection it presented a narrowed appearance transversely, which was due to an increased curvature of the ribs behind, and a slight prominence of the upper fourth of sternum. Resonance was slightly diminished on percussion anteriorly over both lungs. What appeared to me like a dry crackling was heard on auscultation anteriorly over both lungs, but was most intense over the right; otherwise the air entered freely. The percussion note posteriorly was normal, and the vesicular murmur clear and weak. The area of cardiac dulness was increased towards right side, and made the organ seem as if placed vertically. The cardiac beat was not visible, and the impulse was indistinctly felt towards mesial line. The first cardiac sound was very weak, and the second was not audible. The jugular veins were distended, and I think if they pulsated I would have noticed it. I ordered her to continue her ammonia mixture, and put her on—

R.—Pil. hydrarg. gr. xii.

— scillæ 3ss.

Pulv. ipecac. gr. iv.

M.—ft. pil. viii.; one to be taken every third hour.

October 20th.—Had a bad night, but feels a little easier since morning.

21st.—Discontinued pills.

23rd.—Slept for a few hours; starts out of it to catch her breath; cannot lie on either side; is supported by pillows in a sitting posture. The first sound of heart is like the second; the latter is not audible.

28th.—Feels a little better; to take two of the following pills until bowels are moved:—

R.—Ext. belladonnæ, gr. i.

Pil. aloes co. 3i.

M.—ft. Pil. xvi., take two every fourth hour until bowels are moved, and an ounce of the following mixture every fourth hour:—

R.—Liq. fer. perchloridi, ʒiii.

Spt. æther. nitros. ʒiii.

— chloroform. ʒii.

Aquæ ad ʒviii.

M.—ft. mistura.

October 29th.—Improving; is in better humour; asking for food; ordered her the following— ʒss. to be taken whenever the breathing gets bad, and to be taken every hour until relieved:—

R.—Ammon. carbonat. gr. 40.

Spt. chloroform.,

Tr. digitalis, āā. ʒi.

Tr. scillæ.,

Liq. Hoffman., āā. ʒii.

Inf. senegæ, ad ʒvi.

M.—ft. mistura, st. ʒss.

During the day she was visited by some friend, who informed her of some unpleasant news, and at night she had another paroxysm of asphyxial symptoms, which lasted between two and three hours. In the middle of the night she was attacked with a severe pain in the abdomen, which was relieved by a haustus opii; she afterwards accounted for it by eating potatoes.

30th.—Had the best night's rest since her illness; face seems a little puffed.

November 1st.—Feels better; expectorated a little frothy mucus at night before going to sleep; cough, very slight; feet and legs slightly œdematous; hepatic dulness increased; urine, spec. gravity 1015, of a light straw colour—becomes slightly cloudy on heating; pulse, 140—small and irregular; put her on quinine and iron three times a day.

8th.—Feet and legs very much swollen; heart's action stronger; second sound still inaudible.

9th.—Had another paroxysm at 6 p.m., which lasted about an hour and a half; felt great relief from a glass of punch that was given to her. I saw her when beginning to recover; she was sitting up with her arms extended, and pushing against the seat on which she was. The least movement hurried her breathing, which was 56 per minute, and she could not attempt to lie down; she frequently inquired if she were dying; the countenance was livid, the surface was cold and covered with a clammy sweat. Temperature in axilla 98.3° Fahr. Heart's action irregular; pulse likewise, and scarcely perceptible, 140. Dulness on percussion over upper fourth of sternum, where prominence exists; over other portions of anterior surface of lungs resonance is slightly diminished; area of cardiac dulness the same as I described before. A clear crackling is audible over the apices of lungs; can hold her breath without much difficulty to let me examine heart.

12th.—Pulse 110; temperature 98.2° ; respiration 28; heart's action irregular. I thought I heard a whiff over pulmonary artery with first sound. Slept in recumbent position during the night for first time. Before going to sleep she nearly fills a pocket handkerchief with frothy clear mucous expectoration.

13th.—Could not detect any bruit over pulmonary artery or heart.

14th.—Slept well on left side for first time. Pulse 144; respiration

28. Bowels have been kept regular by supporting the abdomen, which was very pendulous and lax, and by the use of figs. Dulness over upper fourth of sternum less; respiratory murmur clear, except over apex of left lung, where there is a slight sibilant rale.

17th.—Put her on the following mixture:—

R.—Liq. arsenicalis, ʒi.

Tr. digitalis, ʒiii.

Aquæ, ad ʒvi.

M.—ft. mistura st ʒi. ter die.

19th.—Heart's action more regular, 144; is disturbed at night, as she is passing large quantities of light straw-coloured urine.

22nd.—Pulse 120, regular and full; second sound of heart audible; area of cardiac dulness diminished; swelling in face, feet, and legs less.

24th.—Has been up for a few hours.

25th.—Went into sitting-room, and left off binder.

26th.—Feels a pain under lower angle of right scapula; small portion of posterior surface of lower lobe of right lung slightly congested. Applied cataplasms of linseed meal.

27th.—Feels better; congestion of lung gone.

Dec. 1st.—Up every day since my last visit; swelling completely gone from feet and legs.

22nd.—Looks quite well; has a good appetite; wants me to allow her to take some wine or porter; has frequently asked me before. All through her illness she did not get any except when she had the paroxysms of asphyxia, when she was allowed small quantities of whiskey punch. Pulse 100, small but regular; heart's action normal, as also the cardiac area; tongue clean; bowels regular.

Jan. 12th, 1878.—Has been out; walked three miles a few days ago; never felt in better health, and has not the least liking for any sort of alcoholic stimulant; is determined to eschew their use, although she never took them in excess. Two of her brothers died, I understand, from chronic alcoholism.

The catamenia returned on Dec. 26, a little beyond three months since the birth of the twins.

Professor Hayden, in his work on "Diseases of the Heart and the Aorta," at page 479, mentions an authority in support of pregnancy being a cause of hypertrophy of the heart; and at page 480 he states that the enlargement, like that of the uterus, is only of temporary duration, and subject, like it, to consecutive involution; and, according to Austin Flint, anæmia and functional debility favour dilatation of the heart.

The period that elapsed from the delivery of my patient until the commencement of her subsequent illness was thirty-four days, although,

observe, she stated during that time she did not feel quite herself, and the nervousness and palpitation began to return. Why the heart had scarcely time to assume its normal dimensions—that is, presuming it had been hypertrophied, and perhaps it had been more so than in ordinary parturient women—at least it ought to be in consequence of her bearing twins.

Her nervous and anæmic condition favoured the occurrence of dilatation, and the right heart on my examination was engorged, and the chambers of the left one nearly empty. This woman had an hereditary tendency to asthma on the mother's side, and the amelioration of the symptoms of carbonic acid poisoning, at the very time you expected dissolution was going to take place, looks as if there was something of a spasmodic character in the attacks.

Niemeyer, in revised edition, Vol. I., page 89, states in the article on Nervous Asthma that the moment signs of carbonic acid poisoning develop, the spasmodically contracted bronchial muscles are relaxed, as are also the muscles of the entire body, upon which the poisoning undoubtedly has a paralysing effect.

It struck me also as rather curious that she could hold her breath with such comparative ease during the asphyxial paroxysm in order to let me examine the heart. During the attacks, and for some time after them, the secretion of urine, although normal, was very scanty, but the combination of liq. arsenicalis and tr. digitalis which I ordered for her on November 17, 1877, seemed to increase it very much. Up to the present I am rather sceptical as to the nature of the lesion or lesions which produced the asphyxial paroxysms, but think it only fair to inform you that I had an hypothesis which might account for them. However, it is completely upset by the anatomical fact that no muscular fibres have, I believe, been discovered up to the present in the capillaries of the pulmonary artery. Had such fibres existed I would have been inclined to look upon spasm of them as the primary cause, the result of which, acting backwards on an hypertrophied and softened heart, produced engorgement of the right side of it, followed by the usual sequences of such. Might not spasm of the arterioles of the pulmonary artery account for the symptoms?

The asphyxial paroxysms diminished in duration as they occurred, but assumed a periodic character by the first taking place on October 19th, the second on October 29th, and the last on November the 9th.

A thrombus of the right side of the heart or an embolus of the pulmonary artery looks a very plausible cause of the disease, the paroxysms being accounted for by emboli of the branches of pulmonary artery; and the bruit I thought I heard on Nov. 12, but which disappeared by next day, might also be accounted for on one of the above suppositions.

The CHAIRMAN observed that the case communicated by Dr. Doyle was of interest because of the attention lately directed to cases of simulated disease of the heart. He referred to a paper on this subject by Dr. Fothergill in the last number of the *Edinburgh Medical Journal*, and remarked that they were often led to jump conclusions as to the existence of fatty heart which afterwards turned out to be erroneous.

DR. MACSWINEY looked upon the case as one of cardiac asthma. He would not have inferred fatty disease of the heart.

DR. HENRY KENNEDY would describe the patient as a nervous female of asthmatic tendency, with temporary dilatation of the right side of the heart. Alluding to the absence of the second sound of the heart, which Dr. Doyle had mentioned, Dr. Kennedy asked where the sound of the pulmonary artery was to be heard best? He thought Dr. Balfour, the latest writer on the subject, placed the point of auscultation too high.

DR. FINNY said he did not go with Dr. Doyle as to the possibility of spasm of the pulmonary capillaries causing the symptoms; but Dr. Doyle had alluded to what he (Dr. Finny) thought was the explanation of his case—namely, the existence of thrombus of the right auricle extending into the ventricle—but in such a manner as if Dr. Doyle did not believe it to be a satisfactory one. The whole *rationale* of the case he (Dr. Finny) thought pointed to a thrombus of the right auricle. The reasons for this opinion were threefold—First, there was the condition of the woman, with respect to pregnancy, when the blood was in a state of hyperinosis, that condition being one which tended to produce phlebitis and phlegmasia alba dolens. He presumed she was not nursing; and the involution of the uterus, which went on after pregnancy with twins, threw more work on the blood, the usual safety-valve of lactation being closed. Thus the blood retained a great deal of effete matter, and was in a condition of hyperinosis. Having regard to the weak and anæmic condition of the woman, it was probable that the thrombus began at the right auricular appendix, and gradually got larger; and that in October it was temporarily dislodged and a portion of it carried into the circulation. The same thing was likely to have occurred when she subsequently heard the exciting intelligence which gave her another bad turn; that was likely to have caused the dislodgment of another portion of the thrombus. The portions of the thrombus thus dislodged became emboli in various parts of the lung. The fact of respiration being feeble in the anterior portion of the lung, and being followed by the clicking sounds heard by Dr. Doyle, confirmed the idea of a number of emboli being in various parts of the lung. Dr. Doyle's treatment was quite suited to this view of the case, ammonia being particularly calculated to keep the blood in a fluid condition, and to prevent the excessive development of fibrin. The rapidity of the respiration was entirely dependent on the amount of obstruction in the pulmonary vessels; and, accordingly, when it came

down to 28, the pulse also came down; and the swelling of the feet, hands, and eyelids, pointing to obstruction at the right side of the heart, abated. He referred to two cases that had been under his care, which presented similar symptoms to those described in Dr. Doyle's case, and which he explained in the same way. With regard to the absence of the second sound of the heart, they knew that there were two vessels in which that second sound was produced, whatever the exact site of those vessels might be. No matter how the second sound of the aortic valves might be diminished, on account of the small quantity of blood passing out of the ventricle, there was no reason why the pulmonic sound should be interfered with, unless the thrombus ran out into the pulmonary vessels as well. With respect to the questions asked by Dr. Kennedy, it had been his (Dr. Finny's) practice to teach that the pulmonary sound was best heard at the upper border of the third rib in the second left intercostal space, and the aortic sound in the right second intercostal space, notwithstanding that the valves themselves were on a level with the fourth sterno-costal articulation.

DR. DOYLE, in reply, said he must concede to Dr. Finny the merit of having given a very clear explanation of the phenomena in his case. As Dr. Finny had said, he (Dr. Doyle) heard the second pulmonary sound below the second rib to the left of the sternum; and he also agreed with Dr. Finny in thinking that the cause of its disappearance was the extension of the thrombus into the pulmonary artery. There was, no doubt, an asthmatic tendency in the patient, but it was not an ordinary form of asthma, there being no constriction of the bronchial tubes, sibilant râles, or wheezing. The patient had never suffered from asthma before the first of the paroxysms by which she was attacked; and there was an interval between the second and third of them of ten days.

The Society then adjourned.

SULPHUR IN THE TREATMENT OF ACNE.

FIVE grains of iodide of sulphur with eighteen grains of the extract of bitter almonds are to be made into ten pills, of which at first one should be taken every day, and then two or three according to the tolerance established. Moreover, night and morning lotions are to be locally applied, consisting of tepid water, to which has been added a tablespoonful of the following mixture:—Tincture of benzoin, sulphuret of potash, of each two drachms; distilled water, six ounces. If the iodide of sulphur should not be well borne, iodide of potassium may be substituted, pushing it to large doses. The skin also may be stimulated every night by rubbing in fresh lard, to which half its weight of sublimed sulphur has been added.—*L'Union Médicale*.

S. W.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

FORTIETH ANNUAL SESSION.

Saturday, 12th January, 1878.

MR. DARBY, President, in the Chair.

The late Dr. Stokes.

DR. M'CLINTOCK.—Sir, I think we should be wanting in self-respect if we allowed this, the first meeting of our Society that has taken place since a great and illustrious member of the medical profession of Dublin has been removed from amongst us, to pass without a resolution on the subject. I therefore submit the following resolution to this meeting for adoption, in reference to the decease of Dr. Stokes:—"Resolved, that this Society, in common with the other medical and scientific bodies of this city, desires to give public expression to its deep regret at the decease of William Stokes, M.D., Regius Professor of Physic in the University of Dublin, and to record its testimony to his pre-eminent abilities as a physician, to the distinguished services he has rendered to the Medical School of Dublin, and to his great qualities of head and heart, and that the sincere condolence of the members of the Society be respectfully communicated to his family."

DR. ROBERT M'DONNELL, President of the College of Surgeons.—Sir, it is not necessary to say more in seconding this resolution, than that I do so most heartily and sincerely.—The resolution was passed in solemn silence.

Multilocular Ovarian Tumour.

DR. MASON.—This specimen which I exhibit to the Society is one of a multilocular ovarian tumour, which was removed this morning by Dr. Kidd, from a patient in the Coombe Hospital. It presents some points of interest. When the abdomen was opened and the tumour exposed, a thick round band was found running obliquely on the surface of the tumour, from about midway between the umbilicus and pubes downwards, and to the right side into the true pelvis. It was not at first evident what this was, but when fully exposed it was found to be the pedicle. The tumour was universally adherent, and had become rotated on an oblique axis towards the left, so that the attachment of the pedicle which was originally on the right side of the tumour, had been drawn upwards and forwards to the mesial line, till its insertion was midway between the umbilicus and the pubes. This rotation was probably caused by the contraction of the lymph forming old adhesions; it was also adherent to the small intestines, and throughout the true pelvis. At the left side of the tumour there was a small piece of bone about an inch square in

the cyst wall. Portions of bone are often found in piliferous tumours of the ovary, but, I believe, are very seldom found in true ovarian cysts. A large amount of the omentum was also adherent to the tumour, and had to be cut off and ligatured. During her last two menstrual periods this woman suffered very severe attacks of peritonitis; the last of these occurred on Christmas day, and when we saw her that morning we believed that she would not live; however, she got over them, and Dr. Kidd performed the operation as soon as possible afterwards. According to her history the tumour was eight months growing. She was a servant by occupation, and in every other respect was tolerably healthy, though greatly emaciated.

Successful Cases of Transfusion.

DR. M'CLINTOCK.—Early in the month of last September, I was brought a distance of several miles from Dublin to see a lady who had been taken with the pains of her fourth labour about nine o'clock in the morning. I did not reach her bedside until near two o'clock the same day, when her state was most alarming. After the labour pains had set in, it was ascertained by the attendant that the presentation was preternatural. About eleven o'clock hæmorrhage, of a very profuse kind, nearly came on. When I saw her, at two o'clock in the afternoon, she was insensible, and almost pulseless, and was reported to have had two slight convulsive seizures. When I examined the bed I found that she was surrounded by blood, and the entire bedding and bed-linen were saturated with the same. On making an internal examination, I found a leg in the vagina. With very little effort the child was extracted, but it was perfectly dead; it had certainly been alive within a very few hours. The placenta came off with slight pressure, and there was no more hæmorrhage. She rallied a little just after the child was born, and made anxious but feeble inquiries as to its state; and gathering from the evasive replies given to her that life was extinct, she was so greatly affected by the disappointment that she seemed utterly overpowered and prostrated, and never spoke for several hours afterwards. We at once set about putting dry linen next her, applying warm jars, administering stimulants, and so forth, and every available means were used in order to try to bring about re-animation. There was no hæmorrhage from the time of delivery, and the uterus contracted tolerably well. She got brandy both by the mouth and by the rectum, and heat was applied all over the surface of the body. The trunk and lower limbs were elevated, and the head kept very low; and, in addition, I administered hypodermic injections of sulphuric ether, which appeared to impart a momentary increase of strength and vitality. For five or six hours she remained in this state, fluctuating between life and death, with scarcely a pulse at the wrist, while all the remedies were being patiently and perseveringly

applied, in order to try and bring about reaction, but without any real improvement in her condition being effected. She remained perfectly insensible and never spoke, the surface of her body being cold, and her pulse at times barely perceptible at the wrist. After having waited for six hours, in the hope of a change for the better, and no improvement taking place, it seemed vain to expect that unassisted nature could rally her. It was then thought that the only chance that remained to her was the operation of transfusion, and immediately a messenger was despatched to request Dr. M'Donnell to come out with the necessary apparatus. He reached the patient's bedside about eleven o'clock, that is, *nine* hours after she had been delivered, her condition being such as I have just now described. Blood was at once obtained from the patient's husband, defibrinated, and injected into the arm without difficulty, except that, owing to the extremely anæmic state of the body, it was not easy, even after the integuments had been divided, to distinguish a vein. However, this difficulty was successfully overcome, and about ten ounces of defibrinated blood injected into the right arm. The patient was apparently quite insensible, and offered no resistance to the operation, by jactitation or in any other way. While the blood was being injected into the arm, I kept a careful note of the pulse at the wrist; and, after a few ounces had been injected, I discovered a slight improvement in the condition of the pulse. Nothing untoward occurred during the operation; and, after the blood had been all injected, the arm was bound up, her pulse being then somewhat further improved; and, in the course of a few hours subsequently, decided indications of reaction began to show themselves, and at the end of about twelve hours the restoration was complete. From that time forward nothing could go on better or more satisfactorily than her convalescence in every respect. Besides Dr. M'Donnell and myself, there were also present at the operation Dr. Malachy Burke and Dr. Stanistreet, of Malahide. It is, perhaps, hardly necessary to add that, in the performance of the operation, Dr. M'Donnell used the apparatus called after him, and of which he published a description in *The Dublin Medical Journal*, November, 1870. I may further remark that the above case bears a strong resemblance, in some of its features, to the one recorded by the late Dr. Beatty (*Dublin Medical Journal*, May, 1870), the successful result of which brought the operation of transfusion into so much repute here. In Dr. Beatty's case the operation was not resorted to for *fourteen* hours after delivery; and in the case above related it was done *nine* hours after delivery. This is a very important feature in both the cases, and shows that, in each of them, ample time was given for the natural powers to rally, if such had been possible. Consequently, these two cases may be regarded as affording the strongest evidence to the value of transfusion, after all other known means had been fruitlessly tried to rekindle the spark of life.

DR. MACAN read the following paper by DR. PUREFOY.—By the kind permission of Dr. Atthill, I am enabled to bring before the Society the particulars of the first successful case of transfusion, in the Rotunda Hospital, which occurred last June, while I was in charge of the institution. The patient, a multipara, was admitted at 9 a.m. on the 2nd of June, in a most alarming state of prostration from hæmorrhage, her face being quite blanched and her pulse weak and rapid. The os was only three-fifths dilated, and the membranes still unruptured. She was scarcely undressed, however, when a large quantity of liquor amnii, mixed with blood, escaped, and the head could be readily felt presenting. A binder was applied, and our utmost efforts were directed to supporting the patient's strength and exciting uterine action, by repeated injections (subcutaneous) of ergot and ether, in the manner so ably advocated by my friend, Dr. Macan. As the uterus responded very feebly to these various stimuli, and blood was still escaping in large quantity, it became manifest some more efficient means of hastening delivery must be tried in order to save her life, and, after a brief consultation with Dr. M'Clintock, I determined to try the forceps, though the undilated os rendered their application very difficult, and invested with no little danger the speedy extraction of the child. Dr. M'Clintock afforded me valuable aid by supporting the anterior portion of the cervix while I exerted traction, and we soon had the happiness of finding the os yield to our united efforts, after which delivery was easily completed without delay. The hæmorrhage, however, though arrested for a few minutes, returned to such an extent that perchloride of iron was injected with the usual good effect. Brandy and beef-tea were given by the rectum; a binder was applied, and, at Dr. M'Clintock's suggestion, an elastic bandage was rolled round each leg from the foot to the thigh; and, at the same time, the lower end of the bed was raised. As the bandages caused pain they were removed after the lapse of about half an hour, but I have no doubt as to their good effect while in use. In about an hour afterwards blood again flowed in considerable quantity from the vagina, and, almost in despair, I injected perchloride of iron into the vagina simply, and changed the binder. Some draining continued after this for awhile; but, though it had ceased, I found her condition at 1 30 so precarious that I sent for Dr. M'Donnell, who, with his wonted kindness, was speedily present; and, as there was now no radial pulse, transfusion was immediately determined on, and Mr. Donaldson, one of our most diligent students, at once volunteered the necessary supply of blood—an act of kindness not a little enhanced by the fact that he was not by any means of a plethoric habit. Immediately afterwards the pulse could be felt at the wrist; from this time no immediate cause for apprehension arose. The vomiting which ensued was checked by using ice and nourishment, and stimulants were given by the rectum.

At 10 p.m. her temperature was $100\cdot3^{\circ}$, and pulse 112. At 9 a.m. next morning I found the pulse and temperature both 100; the woman had slept and made no complaint whatever. In two hours afterwards she had a rigor, and the pulse rose to 150, and temperature $104\cdot3^{\circ}$. Next day pulse and temperature had both fallen and continued to do so steadily until her convalescence, which was fairly established in about two weeks.

DR. M'DONNELL, President of the College of Surgeons.—As I was the person who performed the operations in both cases, perhaps I may be allowed to speak first in this discussion. I am extremely obliged to Dr. McClintock for his statement, which was as clear and graphic as if it had been written. I have also to thank Dr. Purefoy for the case which he has brought forward. I am well aware that doubt must always exist as to whether individuals operated upon under such circumstances as have been so well described might not have recovered even though the operation of transfusion had never been performed. This doubt, however, may be thrown over the effect of any remedial agent as well as this. But I wish to refer to the cases which have occurred in my hands to prove another point. It has been asserted that ill effects—very grave consequences—follow the injection of defibrinated blood. I know no means of refuting this assertion, save looking to the facts. Now I can say that in no one of the cases in which I have performed this operation were there ill after-consequences which could be attributed to it. If it failed, it failed at the time because death was imminent; in no single instance where recovery took place was there bloody urine, or extravasations such as have been stated to occur—in no instance was there, in fact, any alarming symptom to interfere with speedy convalescence. I hope at some future time to lay before the Society a fuller statement of the cases in which I have had the good fortune to have performed the operation with success.

THE PRESIDENT.—Gentlemen, I think it reflects great credit upon Irish surgery that we should have been so successful with this method of transfusion. Other methods have been tried before, but never with such success as to make the operation one to be much relied on. I think it right to state that I saw Dr. Roussell's instrument used. I was at the last meeting of the British Medical Association in Manchester, and heard Dr. Roussell himself describe it, and also Dr. Bennett, of London; and I ventured to say at the time that the instrument appeared to me to be so very complicated that I feared it would be almost impossible to use it in the hurry that takes place in cases of uterine hæmorrhage, where there is no time to be lost. I afterwards had an opportunity of seeing the instrument applied by a very experienced and apparently very expert hospital surgeon, a man of high reputation, who had the advantage of having other surgeons as good as himself to assist him. He had used the instrument three times previously. On this occasion

I observed that it failed in two different ways. One failure was in respect of what I must call the stabbing operation which it involves, in the first instance, for the opening of the vein from whence the supply of blood is to be derived. This is done by a knife with a spring like the fleams with which they bleed horses. A man was got to submit to the operation. There was no difficulty in finding a very large vein; and after the stabbing process had been six times repeated, in two places, it did not open the vein, which rolled away from the lancet every time, although on each occasion the lancet was directly over the vein. Then that man got tired of it, and another volunteered, and after three or four attempts the vein of this second patient was opened. Then the instrument was set to work, and about a drachm of blood was introduced by the stroke of the pump into the vein of the lady; but then the blood coagulated in the small valves of the instrument, and not another drop could be injected. I believe that on this occasion the instrument was used with as much manual skill as Dr. Roussell himself could have employed. The operation was performed in a most painstaking manner, both the chief surgeon and his assistants showing the greatest intelligence and skill.

DR. RICHARDSON.—Did you time the operation?

THE PRESIDENT.—I think we were half an hour over the patient.

DR. RICHARDSON.—That was a serious loss of time.

THE PRESIDENT.—The gentleman using the instrument told me that he would write telling me the result of other trials; and he informed me that it failed in two other cases.

DR. HENRY KENNEDY.—I agree with Dr. M'Donnell as to the propriety of performing this operation even in cases in which we believe that the patient is likely to go on to death. I may remark that three or four years since a most valuable communication was brought forward in London showing that during a cholera epidemic transfusion had been employed with great success. As much as 80 ounces of fluid was thrown in in several cases, and with perfect success, and the patients recovered after they had been pulseless for hours. The only case in which I was ever engaged in the way of experiment upon a living animal bears out the view to which Dr. M'Donnell has alluded. A dog was bled from the jugular vein until he was apparently dead, and lay on his side. A quantity of tepid water was then thrown in, and to my amazement the dog got up and was able to stagger about, and finally recovered. I am quite sure that under the circumstances to which Dr. M'Donnell has alluded we would be quite justified in throwing in tepid water.

DR. MACAN moved the adjournment of the discussion to the next meeting, which was seconded by Dr. CRANNY, and agreed to.

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

SESSION 1877-78.

President—G. F. WALES, M.D.; F.R.C.S., Ed.

Hon. Secretary—WILLIAM WHITLA, M.D.

Tuesday, the 11th day of December, 1877.

The President, DR. G. F. WALES, in the Chair.

Adjourned Debate on Alcohol.

DR. CUMING, in opening the discussion, said:—The first thing he should be obliged to ask them was to restrict the discussion to the terms of the query. The terms had been selected with a great deal of judgment by the Council of the Society. The subject was one on which the members of the Society might be fairly asked to give their opinion, and outside that they ought not to travel. The terms were—"The moderate use of Alcohol—is it injurious or beneficial?" In the first place that clearly excluded altogether the question of intemperance, and left it as a matter perfectly foreign to the essence of the subject, and he was specially anxious that they should leave this out of consideration, because it introduced an element so much debated by the public, involving so many moral considerations, and on which the feelings of many were so strongly excited, that it would be very difficult to prevent their judgment being influenced in one way or other.

He thought that they should devote their investigations mainly, if not exclusively, to what the influence of the moderate use of alcohol was on the average human being in civilised life, and that they ought not to attach too much importance to merely exceptional cases—for instance, those individuals who had such a passion for alcohol that they could not restrain themselves from excess if they once tasted it at all. Nor need they dwell on those exceptional instances in which a small amount of alcohol produced a very great effect on the nervous system. These might be looked on as the idiosyncrasies, and had to be treated as exceptions, such as occurred in the use of morphia or mercury. In dealing with the broad general fact, they should examine it on physiological grounds, as they would examine how castor oil purged or chloral produced sleep.

In addressing an audience such as that before him, one might fairly assume that all the principal experimental evidence was tolerably familiar to them. They need not argue about theoretical considerations. The

great question was—what was their own experience, their own observation as medical men? They were all agreed, firstly, that intemperance was a thoroughly bad thing. He was sure they were all agreed that a person might preserve the highest amount of physical health without taking any alcohol at all. That had been demonstrated so very fully by temperance societies, no one could doubt it. Then came the main question—was alcohol in moderate quantities useful? He knew when he was entering on this subject, and undertook to discuss it at all, that he was touching on dangerous ground, but he felt that it was in a sort of way a duty he owed to the profession not to hide his own views. Now what were the conclusions that his own experience had led him to. He took the average man as he found him in his practice amongst the mercantile and professional classes, whose duties entailed a great deal of wear and tear, a good deal of expenditure of energy, and frequently considerable physical as well as mental exertion. What he believed he had observed in these was, that the use of a small amount of alcohol taken at the evening meal did the following things:—In the first place, it gave a certain amount of appetite to men whose appetite was occasionally doubtful, capricious, and possibly deficient. Again, he thought it sharpened the appetite of men when it was flagging; and, thirdly, he thought that it prevented or diminished the unpleasant sensation of fulness after eating, which was very common in those classes. He was speaking in this matter of averages. He knew there were many exceptions, many men who did very well without alcohol, but he spoke of the general run of men. Persons who were very much opposed to the use of alcohol in any form might meet this by admitting that alcohol can produce these effects, and following it by the statement that the effect passes off soon, and that there ensues a reaction which is equivalent to the primary stimulating effect. It might be said that it was quite true that there was an apparently beneficial effect at the time, but that that effect passed off, and that the patient sunk as far as he had been elevated—

“As high as we have mounted in delight,
In our dejection do we sink as low.”

This, in his own opinion, was not the fact. He thought that a small amount of alcohol did not lead to any injurious reaction. The question then arose—what was moderation in alcohol; was it possible to lay down a rule for moderation? and Dr. Moore had dwelt on this subject with a great deal of force, and urged that the difficulties in the way were such that it was not possible. It was difficult to lay down any rule, and impossible to lay down a strict rule; but there was no more real difficulty in making an approximation as to what was a quantity of alcohol than there was in ascertaining what was moderation in eating. If a person took such an amount of alcohol as in any way trammelled his mental or physical energy, or produced an uncomfortable effect on his sensations, or if he took

an amount beyond what he found others to use with safety or advantage, then he knew that he had passed the standard of moderation, and, he thought, that that standard should be fixed at a very moderate amount. He believed alcohol did more than this. He thought a small amount taken by the average human being living in a high state of civilisation had the effect of blunting his susceptibility to petty annoyances. It had something of an anæsthetic property. He thought it also to be to many a source of harmless gratification, and he did not think the pleasures of life were so very numerous that they should altogether ignore a consideration of this kind. Having gone over this part of the subject he must meet another objection made by total abstainers—that was, they would admit that these beneficial effects might be produced, but then they said the amount that would produce them to-day would not do so to-morrow or months hence; that they were on the edge of an incline; that in a year or so the quantity had to be increased, and so on until it went beyond the limits of moderation, and into the regions of intemperance. Undoubtedly this was a fact in many instances, but these instances he thought also to be exceptional; and, after looking over and very carefully considering this point, he thought from the observations he was enabled to make amongst the men he had known, and with whose history and habits he was familiar, that such a quantity as was taken for a beneficial effect did not require any addition whatever; and he thought that, so far from an increase being necessary in that way, the answer he would be inclined to give was that the experience of humanity was that rather a diminution was the rule than the exception. They saw many instances of this on a large scale on the Continent of Europe, especially amongst the French, Spanish, and Portuguese and Italians. In these countries the inhabitants were accustomed to take a moderate amount of stimulants all their lives, and they were noted for being generally temperate. Whilst he was quite clear with regard to these views as generally applicable, still it had to be borne in mind that there was a considerable portion of the human race who were gifted with such special nervous organisations that the use of alcohol with them was attended with greater risk than with the generality of the community. Those who were subject to the complex nervous sympathies which were grouped together as hysterical manifestations seemed more liable to the injurious effects of alcohol than their more fortunately circumstanced neighbours, while they were less benefited by it.

He had only to allude finally to the position of the profession with regard to a question of this kind. All of them would recognise what enormous evils had been produced by intemperance, what misery and penury, how fertile a source it was of crime, of vice, and of physical degradation. They must be obliged to admit all this, but did that alter the physiological effect of small quantities. He did not think they should

be influenced in arriving at a conclusion by considerations of this kind. They had to hold by scientific truth no matter what was the result. There seemed to be in the minds of many a fear that if they admitted any benefit from the moderate use of alcohol they were opening the door to intemperance and its evils. He was not sure that there was not some element of truth in this, and his sympathies went with those who advocated total abstinence. His judgment and experience were on the opposite side. When they were asked by a patient would a moderate amount of alcohol be beneficial or injurious to him, he thought they should give a candid answer, without taking into consideration what influence his example might have on his neighbours, or what were the evils of intemperance in the world.

PROFESSOR DILL, M.D., after stating that he would not confine himself to the rules laid down by Dr. Cuming, which he thought hampered discussion, said:—In arguing this question, I am disposed to lay it down as a *principle*—for at least *my own guidance*—that Science and our first great Authority are never found antagonistic to each other; and starting, as I do, upon this principle, we shall see how it may be brought to bear upon the question at issue, in its social, its moral, and its therapeutical aspects. If what has been stated be admitted, then I go further and assert, that as the use of fermented liquor is sanctioned and recognised for the use of man, we cannot step in to condemn the legitimate and temperate use of it, unless it may be on the ground of expediency. This being so, it must be admitted that it is the abuse and not the use—moderate use—of this agent which is to be discouraged.

I would, moreover, from a social standpoint, be inclined to say that, except where individuals offend against *society* or against *self*, or are in any way disposed to so offend, the *law* should not be put in motion.

I have been at all times an advocate of the very moderate use of alcoholic drinks, and I would still hold by the same opinion. But when I see the daily abuse of it becoming such a plague-spot on our society, then I am forced to the conviction that a reform is urgently needed. On this point I do not enter into details, neither is it necessary that I should here be more specific.

I have said that the question is a vexed one, and it is made more so by the discrepancies and the discordant views entertained by scientific investigators, and amongst medical men themselves. We have Dr. B. Ward Richardson publishing his popular tracts and delivering lectures upon the subject to the general community, sowing broadcast the seeds of his psychological dogmas, and inoculating the untrained mind in science with his fallacious arguments. I shall only take leave to say here, that there is evidence enough in his own statements for a satisfactory reply, and I

believe that his whole argument applies to the abuse, and not to the use of alcoholic drinks.

Give me leave to offer you a specimen of Dr. Richardson's reasoning. He says that because the lower animals refuse the use of alcohol, *ergo*, it is not food; it is not suited to man, and should be rejected by him. As well might I advance as an argument the fact that, because my horse refuses to eat cabbage, *ergo*, cabbage is not food; it is evident that this animal cannot look upon this article as a favourite cordial. But we have as an offset to Dr. Richardson's theory on this subject, the opinion of one of the ablest of scientific investigators—I mean Dr. Burdon Sanderson, who maintains that alcohol is a food, and may be used as such in moderation. My own experience leads me to the same conclusion, and that it is to be considered, under certain circumstances, as assisting in promoting recovery from disease.

I have now passed through many a phase of life and medical thought, and some very extreme opinions on different subjects have been advanced and practised, and again more moderate views appeared to receive greater favour. I more particularly allude to the extreme views entertained at one time in regard to both the excessive use of stimulants as recommended by the late Dr. Todd, and the very free use of the lancet. These two extreme views are only known to history, and still the question recurs to us, "What of a little wine for the stomach's sake and for our oft infirmities?" to which I would reply, that I have no more right to condemn the general and moderate use of wine, than I have to condemn the proper use of bread and butter and beef.

He went on to say:—I have come to the conclusion that the stimulating plan of treating certain diseases is not so necessary as some would have us to believe. I am also of opinion that the symptoms requiring its use are not watched for with sufficient care, which symptoms are clearly told to us by Dr. Stokes—viz., when the heart's action begins to show signs of failure. The late Dr. Graves has handed down to us an expression of opinion in these words, that "he fed fever;" and, I have no doubt that to many, simple though these words may appear, they are too freely and literally interpreted.

I have in my own person experienced the disadvantage of taking brandy while making a long journey on the top of a coach, on a cold winter night, in the extreme fatigue felt next day. I made the return journey a couple of nights afterwards, on which occasion I took nothing of the kind, and I was comparatively free from fatigue on the next day. I have found that from half a glass to a glass of spirits will arrest the pains of labour as effectually as will two or three grains of opium. I have found that drinking habits are occasionally associated with disease of the womb, but I have not discovered whether it is the disease creates the habit for the drink, or the drink brings on disease.

I am also of opinion that drink creates an increased sexual desire; and do we not know that the poor "unfortunate," by her indulgence in alcoholic drink (as is usually the case), gets into a condition both of obesity and barrenness.

I have often been surprised at the very great quantity of drink that some persons will venture to take—a pint, a quart, two quarts of whiskey, are no uncommon allowance over the day.

One would think that the words in the *Spectator*, by Addison, were applied as a delicate satire on the drinking habits of the age—"I am well, I would be better, and here I am."

Ah! sir, I have often wished to see some fair hand of genius arise which would restore these matters to their proper level, for when in our highest degrees of enjoyment and happiness, are we not cast down to the lowest depths of depression, as we contemplate the sorrow and the suffering which that demon by his excessive use of drink has entailed, reducing this fair earth of ours to another Pandemonium.

The discussion was then adjourned until Tuesday, December 18th.

Tuesday, the 18th day of December, 1877.

The President in the Chair.

Adjourned Debate on Alcohol.

The debate was re-opened by DR. HENRY MACCORMAC, Consulting Physician to the Royal Hospital, who said:—The common sense and experience of mankind have long since decided the question as to the moderate use of alcohol. The properties of alcoholic drinks were known from the period—and that is now a long time ago—when the first man or woman laid aside for a little the pressed juice of the grape. Alcoholic drinks, without any reasonable doubt or question, are among the immense gifts of God, and when used with proper restraint and measure, are, or may be, productive of great good. This premised, I must now say that the abuse of alcoholic drinks was, and is, the source of prodigious evil. In fact the community are unaware of the ethics and physiology of fermented drinks, and would require very careful training in regard of both. There are multitudes, we must regret to find, who do not look upon alcoholic drinks as a source of partial sustenance and refreshment, but merely as one of animal enjoyment. The man or the woman who drinks with the express object of intoxicating himself or herself must be considered, in this respect, at least, as little better than a brute. There are at the present moment some 150,000 public-houses in Great Britain and Ireland—all of them potential, and very often real, arenas of destruction and death. In these houses, men, and even women, spend the proceeds

of their industry, robbing their families, debarring themselves of proper food and clothing and lodging, wasting their health and strength and time, and rendering a provision for age, or illness, or accident, simply impossible. It could not well be otherwise when we find that £147,000,000 sterling are yearly paid across the counter for alcoholic drinks. The present earnings of the United Kingdom amount, it is said, to £1,200,000,000 annually. And were these earnings not so great, it would be impossible for British energy, prodigious as it is, to withstand so immense a drain. As it is, there are a million or so of paupers—paupers reduced, in too many instances, to pauperism by excess and the almost utter absence of needful forecast. The gaols are filled with criminals, asylums for the insane with mad men and mad women, hospitals and the houses of the poor with preventable disease, more or less springing from the same cause. It is infinitely to be regretted that the fiscal arrangements of the country are such as to render it needful, or seemingly needful to derive a revenue, or at least so great a revenue, from the sale of intoxicating drinks. Would it not be better to tax property, or resort to other means to compass the requisite funds? Instead of holding out virtual boons for intoxication, every possible incentive should be yielded to the cultivation of habits of prudence, sobriety, industry, and forecast. Compulsory education should be rigidly enforced all over the three kingdoms as well as in the colonies. Primary, intermediate, and technical schools, with lending libraries, should everywhere abound. The Legislature, if it would, might, so to speak, put down intoxication in a day, for it has the power. Seventy or eighty years ago claret cost some £12 the hogshead—the price at which it may be purchased now. Then a war tax was levied, and the duty on imported wines raised to the monstrous amount of five shillings and upwards per gallon. Then began the introduction of brandied wines, and heady, unwholesome beer. The employment of such beverages, coupled too often with the habitual use of spirits, raw or haply diluted, instead of light wines and mild ales, has proved most disastrous. People unable to afford the one were content to rasp their palates with the other, and all the evils of intoxication followed in their train. The mild light wines and pleasant beers of France and Germany, so harmless and refreshing, do not contain—the former more than from seven to eight, the latter from thirteen to fourteen per cent. of alcohol; whereas, not to speak of fiery ales and heavy porter, sherry, port, Madeira, and Marsala are so dosed with brandy as to contain thirty, and even upwards, per cent. of alcohol. These alcoholic wines and heavy ales are really quite unfit for general safe consumption, since they are calculated, in too many instances, to lead to intoxication and disease—cannot, in fact, be largely partaken of without danger to sobriety and every prudential economic check. The employment of raw spirits, so very much resorted to by masses of the population—I am sorry to

add, of both sexes—is a pernicious, not to say savage, addiction. The natural, unbranded light wines of France, Hungary, and Germany, when unadulterated—say half a pint, a little more or less, at a meal—are absolutely uninjurious. I cannot speak too highly of Bavarian beer, and the light unintoxicating ales, generally, of Germany, Holland, and, indeed, those of France. They may be consumed in moderation by those who like them, with perfect safety, and I could earnestly wish to see them at the home table of the working man, and, as a support, to the nursing mother. In other respects fermented drinks of any kind should commonly be alone employed at mealtime. A prohibitory tax ought to be levied on brandy, whiskey, gin, and rum, whereas the duty on light Continental wines, unbranded, should be lowered from a shilling to a penny or two per gallon, with commensurate abatement in the malt and hop tax. The half-pint of rum allowed daily, per head, in ships of war, should be changed for a half-pint of claret. The captains and officers do not drink rum, and I do not see why the men should. It only leads to evil habits. The German mode of brewing beer I would everywhere enforce by law. Sophistication of all kinds should be carefully guarded against. Municipalities should keep the public-houses in their own hands, and pay a fixed salary to the salesman, as is done at Gottenberg. These measures, if carried out by the Government in the first instance, and municipalities under them, would go far, I submit, to abate the ravages of intemperance, and render the moderate use of the great good gifts of the Divine Creator innocuous absolutely.

DR. JOHN W. BECK addressed the meeting as follows:—This question would appear, *primâ facie*, a very simple one, and one of very easy solution. That the use, and particularly the moderate use, of anything which all mankind have been in the habit of using, when they could procure it, must have been in some sense beneficial, would appear to be almost self-evident. Indeed, this universal consent would seem to indicate that it was beneficial in all senses, and not at all injurious. But the question is not so simple as this. It is surrounded with difficulties. In starting for a conclusion we meet with so many modifying circumstances and disturbing elements, each of which must be taken into account, and each of which is so difficult to estimate as to its value, that we feel almost inclined to despair of being able to come at anything like a correct answer to the general question. The answer, in any individual case is not so difficult, as the data for a correct conclusion are not only fewer in number, but much easier to be correctly ascertained. Still it requires great consideration, and will tax the skill of the best of us in any case. The first thing that meets us in this inquiry is the great difference in the opinions which have been formed, with regard to alcohol itself, by persons claiming to be experts. We find *soi-disant*, very high authorities setting it down as a “deadly poison”—whatever that may mean—compounded in Pandemonium: pro-

nouncing, *ex cathedra*, everything connected with it, at all times, in any quantities, and under all circumstances as evil, and only evil. Others, not so noisy, but I would say, with all due deference, of at least equal authority, have pronounced it to be a food, or at least a condiment, or both. It has been set down both as a stimulant and as a sedative. On the one hand it has been accused of causing degeneration and destruction of the tissues; on the other hand it has been extolled as one of the best means we possess of preventing waste of tissue, such as occurs in fevers and phthisis, or after long and violent exercise; and this, not only by inducing sound sleep, and thus putting the nervous centres into the best condition for recovering their own exhausted energies, restoring tonicity to the muscles, and vigour to the whole system, but also by supplying material for combustion in the lungs, and thus saving some more valuable hydrocarbon. I ask why do we find this wide difference of opinion among men equally in search of truth, and we must suppose equally capable of finding it? Notwithstanding this—not too charitable supposition—I suspect the solution of the difficulty lies in the fact that alcohol is all these things, and produces all these effects, and a great deal more besides. Hence the difficulty of dealing with its use in health as a general question. To come more immediately to the question before us—the first point to determine is, what is the moderate use of alcohol? Can this question be answered with any degree of accuracy? If we cannot come at the truth, let us endeavour, in the language of my eloquent friend who opened this debate, to vibrate or oscillate as near it as possible. I conceive that in proportion to the correctness of the answer we can give to this question will be the value of any conclusion we may come to in the matter. It is universally known that what would be a very moderate quantity of alcohol for one individual would be quite an immoderate quantity for another. The glass of punch that might be necessary, of an afternoon, to make an octogenarian feel comfortable, help him to digest his dinner, and procure for him a good night's sleep, may be quite sufficient to make a younger man not only very uncomfortable in the evening, but rise with a very sore head in the morning. I cannot at all agree with Professor Cuming that the quantity of alcohol which the average man—idiosyncrasies apart—may imbibe with safety, or benefit in the twenty-four hours, is anything like as easily to be ascertained as the amount of beef-steak that the average man ought to eat, with good effect, in the same period of time. Any *maitre d'hôtel* will be able to tell to an ounce how much beefsteak a given number of the average man will consume in a given time; but will at once pronounce the alcohol they will imbibe in the same period of time to be an unknown quantity. I told a gentleman a few weeks since to abstain from alcohol altogether while under my treatment. A week or two afterwards he

volunteered the remark that he had abided strictly by my orders in everything, and had been particularly abstemious, not having exceeded three or four glasses each day since he had come under my hands. I give you this as a sample of what some people call, not only moderation, but abstemiousness. But, to come nearer our subject. It has been stated, and I believe correctly, that the system of an ordinary healthy adult could digest, use up, dispose of, or eliminate if you like, about two ounces of alcohol in the twenty-four hours without any trace of injury being left behind.

So far, I think, there seems to be no controversy; but, was it beneficial? Well, if it added to the feeling of comfort of body, and contentment of mind, and left no sting behind, I should say it was—even supposing the process was only elimination, pure and simple. But there is something more to be considered than this. Does this daily dose, of two ounces of alcohol, entail no mischief? It is admitted, on all hands, that during its operation it lightens the burdens of life, gives a brightness to the intellect, an elasticity to the step, and paints the whole world *couleur de rose*. This seems a very desirable state to be in, but can it be made to continue indefinitely, or even for any considerable length of time? The experience of us all at once answers that it cannot. This unusual—I would say this unnatural—strain, action, energy, or whatever else you may choose to call it, cannot be maintained. The system gradually becomes accustomed to the two ounces of alcohol, and the world and its concerns come back, *pari passu*, to their old, common-place jog-trot. You will observe I am supposing the dose has not been increased. Now, the question arises—the question under debate—has this man, in the prime of life and health, gained or lost anything by thus daily dosing himself with two ounces of alcohol? He gained a little pleasure—gratification—for a short time, while the alcohol was new to the system, but that appears to be all. He could have done as well without it. He probably lost nothing, but he risked a temptation to increase the dose. Professor Cuming seems to think that this risk is very small, and that, in the majority of cases, the dose is not increased. It is very difficult to ascertain the truth in this matter; but this is contrary to all my experience. If the alcohol be now dropped altogether for a sufficient length of time, the same process may be repeated again with the same results. Unfortunately, this is very seldom the case. The attempt is generally made to retain and prolong these benefits—and benefits they certainly are, as far as they go—by increasing the dose; but such attempts must necessarily fail. The over-excitement of brain and the surcharge of alcohol which follow this course are sure to bring about a fearful retribution. How few there are comparatively who having habituated themselves to this daily dose of two ounces of alcohol stop there. I have known several, however, who,

having palpably lost any benefits they had obtained from this daily dose, and wisely fearing the temptation drawing them to increase the dose, dropped it altogether. I know several gentlemen who, with the snows of time descending on their heads, abstain altogether from touching alcohol, though for twenty or thirty years of the prime of their lives they indulged in their daily dose without ever exceeding it; and they dropped alcohol for these reasons. Now, this I would call reversing the order of nature. They used—not abused, remark—alcohol when it was little, if any, use to them, and they now abstain from it when it is calculated to be of much value in their declining days; and the more valuable it would be now had they refrained from it in earlier life, but I confess I would be very slow in advising them to touch it again. And now I will say a word on the abuse of, or rather, on the temptation to abuse, alcohol. It is a long time since the wisest of men said, “Wine is a mocker.” It is a mocker. How many have unwisely increased, and continued to increase the dose, till, taken in an unknown quantity, it not only ceased to be a pleasure, but became an absolute necessity for the very existence of their beastly nature. Nay, none can know better than my present audience of a nature reduced far below the level of the beast that perishes. It is melancholy to reflect how many (some of them the finest specimens of men—aye, and the loveliest of women too) have fallen before this unknown quantity, which, at first, in their judgment, constituted “the moderate use” of alcohol; but, once over the boundary of the really moderate use, the rest is easily told: *facilis descensus*. Gentlemen, we are the advisers of men. They look to us for guidance in this matter; and they too often look to us merely to sanction a “use” of alcohol, which they are perfectly conscious is a thorough abuse of it. How difficult it is for a medical man, placed in these circumstances, to act conscientiously, we all know. But here we must be, to a great extent, our brothers’ keepers. Let us ask ourselves, *Quis custodiet custodes?* or, should I not rather say, *custodiret?* It becomes a serious question with us, medical men, whether the beneficial effects of alcohol, such as they are, do not sink into utter insignificance when weighed against the injurious effects produced by the temptation—the amazingly strong temptation—to increase the dose up to the unknown quantity that leads to complete destruction. For my own part, I do not believe alcohol to be either necessary or beneficial in healthy adult life, under ordinary circumstances—say from puberty up to fifty, and, in some cases, sixty years of age; and I am quite convinced, from experience, that females are much more liable to be injured by it than males during this period of life. I need scarcely touch on the use of alcohol in infancy; for, after a practice among infants of more than forty years, I have no hesitation in saying it can be of no use there, but must in every case and under all circumstances do harm. During youth and up to puberty, we are, perhaps, on

more debatable ground; but I hold much the same opinion of alcohol during that period as in infancy—let it alone.

We now come to what I consider the real use of alcohol in health. After fifty or sixty years of age “the moderate use” of alcohol will do no harm, and may do a great amount of good. If judiciously used, its dietetic use in the last decade of life’s years may be very valuable, and it will be found to be much more valuable, as I have before hinted, in declining years, if it has neither been used nor abused in early or mature life. Besides, I hold that the temptation to increase the dose injuriously in any individual who has scarcely touched it at all till after fifty or sixty years of age, is about *nil*. This is no mean consideration to anyone entering on life. In proportion as they refrain from alcohol in early or mature years, in like proportion will they not only reap all the benefits its use is capable of conferring on their declining years, but in like proportion will they also escape from its evils. [Dr. Beck then related an instance where he had treated a patient, an old lady of about ninety years of age, who had been a consistent abstainer during life. Some weeks since she had an attack of diarrhœa, and after trying, first, wine, and then whiskey, neither of which had the desired effect, he suggested bitter ale, the use of which soon produced a marked change on her health and appearance. He said he had no doubt the lady owed the prolongation of her days to alcohol, presented to the stomach in the shape most agreeable to it, and most fit to be digested by it under the circumstances.] In conclusion, he said:—Notwithstanding all that can be said, and truly said against alcohol, I never could see the way clear enough to become so much of a Turk as to eschew alcohol altogether. I have no fault to find either with those who entrench themselves behind the *Kara Lom* of “Teetotalism,” or of those who fortify themselves in the *Plevna* of “Good Templarism”—but I altogether dissent from the conduct of those enthusiastic Turks who will persevere in breaking their heads against the impassable *Shipka Pass* of that craze called “The Bible Wine Question.” Gentlemen, he is a wise man who, in health, can let alcohol alone; but, in my opinion, he is a much wiser man who can touch it moderately, and gently, and carefully, without being seduced by it to injure either his health, or his pocket, or his character.

On the motion of Dr. SPEDDING, the debate was further adjourned until Wednesday evening, 2nd January.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

President—EDWARD HAMILTON, M.D.

Secretary—E. H. BENNETT, M.D.

Congenital Misplacement (Dystopia) of the Right Kidney.—DR. J. M. FINNY showed the specimen, removed from the body of a woman, whose history was unknown. She seemed about fifty years of age, and had lateral curvature of the spine to the left in the dorso-lumbar region. The right suprarenal body was of natural size and in its normal situation, receiving its arterial supply from the aorta. The left kidney was normal in situation, size, and structure. The right kidney lay just above the brim of the true pelvis over the sacro-iliac synchondrosis, the upper half resting on the psoas muscle, the lower projecting as a prominent tumour into the pelvis. The kidney lay behind the peritoneum as it was reflected from the lower surface of the mesentery to the pelvis, surrounded by a small quantity of fat. It was not movable. The right common iliac artery passed under the kidney, causing a slight oblique depression on its under surface; the vessel, which was uncovered in this situation by peritoneum, was one inch and a quarter longer than the left, and was displaced inwards and downwards below the brim of the pelvis. Behind the upper part of the kidney the spermatic vessels lay on the psoas muscle. The kidney, which appeared healthy, was smaller than natural, and was lobulated; its convex surface was directed downwards and outwards; its inner upwards to the spine. In its inner edge was a large hilum, from which proceeded the ureter and the renal vein, and into which passed a large artery, which issued from the abdominal aorta half an inch above its termination and an inch below the situation of the inferior mesenteric. The vascular supply of the kidney was anomalous, and derived from four sources:—1. A branch from the aorta which entered the hilum. 2. A branch from the right common iliac artery as it sprang from the aorta—this vessel passed behind the upper end of the kidney and entered it on its outer surface. 3. A large branch from the sacra media, which entered the back and lower part of the hilum; and 4. A small branch from the right internal iliac just after its formation. There were three veins opening into it; (*a*) the internal iliac; (*b*) the left common iliac before it joined the inferior vena cava; and (*c*) the vena cava, one inch below the point of junction with the right spermatic. The ureter, which was short, passed in front of the lower end of the kidney, and it commenced in a double manner from the upper and lower

parts of the kidney, thereby forming a sling, the common ureter being found an inch from their points of exit. The upper part of the sling was again made up of two lateral ureters, and the lower of three. The displacements of the kidney are either congenital or acquired. The congenital is a fixed displacement. The acquired is migratory. I believe this to be an example of the congenital variety. It is not an example of a displaced kidney such as sometimes occurs when a tumour such as a large right lobe of the liver presses down on the kidney, or where a violent fall or concussion is found to be followed by the same misplacement. The displacements of the kidney, so far as I have been able to learn, may be in several positions—viz., either in front of the abdominal aorta, or, as in this instance, on the edge of the brim of the pelvis, the lower half projecting beyond the edge of the pelvis resting on the sacro-iliac synchondrosis, and making the artery of considerable length on account of the weight of the kidney pressing it down into the pelvis. Besides this there are cases on record where it has been found by examination in the pelvis easily reached by the finger. One case described by Herr A. Hohl was that of a delay in labour produced by the left kidney lying in the pelvis. These displacements of the kidney have never, as yet, been recognised during life. Tumours have been recognised, and various theories have been started, as to what they were; but the exact conditions which indicate displaced kidney have never been sufficiently clear to enable an accurate diagnosis. The practical importance, which at best is small, is in bearing such a condition in mind in the case of obscure tumours, and of such a displacement as this giving rise, as it possibly did, to some of the evidences of aneurism. Very few instances of displaced kidney have been exhibited to this Society. In fact, looking through the records of this Society, I find none of true displaced kidney like this. Two specimens of “horse-shoe” kidney have been exhibited, one by Dr. Stokes and one by Dr. Gordon. In 1841, Mr. Ferrall—as he was then called—exhibited a right kidney, which was displaced so low as the head of the cæcum, forming a tumour which gave rise to great difficulty of diagnosis. An instance resembling the case I exhibit is recorded by Dr. Durham, in Guy’s Hospital Reports, in which the two ureters were united about an inch from the kidney, and the arterial supply was derived from a branch above the bifurcation of the aorta from the common iliac on the opposite side, and from the internal iliac on the same side. In the present example there are more vessels than I have seen recorded in any other case. In the cases recorded, the left kidney is the one most commonly displaced. Dr. Roberts has collected 21 cases of which 15 were on the left side. Of 44 cases given by Weisbach, 35 were displacements of the left kidney, 8 of the right, and 1 a case of the displacement of both kidneys. The same view of the greater frequency of displace-

ment of the left kidney is supported by M. Portal and Dr. Aubé. As to the frequency in males or females, Ebstein, who gives 29 cases, finds of this number 20 to be men and 9 women. The cause of the misplaced kidneys is due to local change or accident during the early period of life, such as may produce a floating kidney, but may be attributable to changes that took place during embryonic life. Ebstein writes as follows (*Ziemssen's Cyclopædia*, Vol. XV.):—"These anomalies are ascribable to a deficient energy in the movements of the embryonic rudiments of the kidney, which, up to a certain time, are situated immediately in front of the bifurcation of the aorta (Kupfer); at the same time the anomalous origins of the renal vessels and the neighbouring vascular trunks, their usually increased number and corresponding shortness, are worthy of notice. These vascular anomalies are easily understood when the rudimentary kidney is bound down to the spot where it originated, or its vicinity (in consequence of some pathological process), as it is very probable that the vascular system gradually developing in it forms communication with neighbouring vascular trunks."—*December 1, 1877.*

Dislocation of the Spine ; Acute Muscular Atrophy.—Dr. T. E. LITTLE, in exhibiting a portion of the spinal column, said: Cases of genuine and uncomplicated traumatic dislocation of the spine are admittedly amongst the rarest of surgical accidents. The existence of such a lesion has been denied, and its possibility even doubted, by some of the highest surgical authorities. I think the specimen I now present, nevertheless, deserves the title. The clinical history of the case it belonged to is of sufficiently curious interest to warrant me in detaining the Society for a few minutes with some of the particulars of it. It was taken from the body of a youth, aged eighteen, who died on the eighty-sixth day after the receipt of an injury, for which he was admitted to Sir Patrick Dun's Hospital. While he was engaged in unloading a coal vessel he slipped and fell down the hold, alighting on a heap of loose coal, upon the back of his head and shoulders, thus apparently doubling forwards the neck upon the trunk, after a manner not unusual in the causation of spinal injuries. He was stunned for the moment, and was shortly conveyed to hospital, but it is worthy of noting that he was able to walk, himself, with the assistance of a couple of his comrades, into the hospital-ward from the cab which brought him, proving that at this stage he had not any significant paralysis of the lower extremities. On examination of him in bed I found the following condition of things:—There was almost complete loss of power of both upper extremities, while (as just mentioned) there existed slight, if any, paralysis of the lower limbs; the sensibility, too, of the upper extremities was considerably diminished; as he lay in bed he held the head slightly retracted, but not fixed or rigid; he complained, however, of intense pain and tenderness of the posterior region of the neck on

manipulation, or any attempt at flexion or rotation of the head; he had passed urine and fæces voluntarily, and without trouble since the accident, and there was no priapism; the temperature in the axilla was 99.5° . After the most careful objective examination of the cervical spinal column, I was unable to make out any evidences of irregularity or displacement of the spinous or transverse vertebral processes, or the slightest indication of crepitus on any justifiable manipulation. Examination through the pharynx also was entirely negative in indications. The youth was, at this time, I may mention, a rather stout and plump lad, rendering even greater than they ordinarily are the difficulties of an external examination of the well-covered cervical vertebræ. A symptom which I have on several occasions before specially noted in cases of injury of spinal cord—viz., a more or less persistent flushing or blushing of the surface of the upper regions of the body—face, neck, shoulders, &c.—was particularly well marked in this boy's case, and lasted for the first week. During this first week the patient's chief complaint was of severe pain and soreness of the neck, back, and shoulders—the pain shooting down the arms, more especially on the right side. He had also, at the same time, a distressing sensation of tingling (like “pins and needles”) in both upper limbs; there was great restlessness and sleeplessness, and the bowels became obstinately constipated, but enemata and purgatives acted fairly well. About the tenth or eleventh day I first observed what I conceive to constitute the most peculiar clinical feature of the case—viz., the supervention of a most extraordinarily rapidly-developed atrophy of the muscles of the forearm on both sides; so wonderfully rapid was the development of this phenomenon, that within the space of three or four days (and the patient had been, meanwhile, carefully examined in detail each day) from its first approach, the atrophy might be said to be almost complete; and at the end of the first fortnight the hands and forearms presented, in a well-marked degree, all the striking and characteristic peculiarities of the most confirmed case of the so-called “*Maladie de Cruveilhier*.” On the fourteenth day I have made this note:—“The ball of the thumb is almost wholly wasted; the peculiar attitude of the hand termed the ‘*main en griffe*’ is typically exhibited; the muscles of the forearm are so wasted that the contour of the bones is quite distinct beneath the skin; the interosseous spaces are deeply hollowed.” Meanwhile, the muscles of the shoulders and upper arms remained apparently intact and plump. About this time, or shortly afterwards, another phenomenon (to which, I believe, as occurring after nervous lesions, Dr. W. Mitchell and other American surgeons have, of late, more particularly directed attention) was noticed—I mean, a peculiar smooth, glossy, and shining appearance of the surface of the fingers and hands, which, I presume, equally with the muscular atrophy, to have a pathological significance pointing in the direction of some serious interference with

the function of the trophic nerves of the part. For a considerable time after this the patient remained very much *in statu quo*; the appetite being good and all the organic functions being apparently performed with tolerable regularity. At one time he was attacked with a curious form of intermittent febrile seizures, marked by very severe rigors and profuse sweatings, occurring daily for about a week, and which yielded during the administration of quinine, which I ordered to him on the faith of some observations made of late by certain surgical therapeutists on what has been termed "paralytic fever." (See *The Practitioner*, July, 1870.—Binz.) Eventually, however, the patient began to emaciate, and the lower extremities became paralytic and contracted, but *without* any muscular atrophy of these members. A few weeks before death he, for the first time, suffered from urinary troubles, the urine having been hitherto always of healthy appearance and normal reaction, and passed voluntarily without impediment; he was suddenly attacked with inability to pass water. This urinary retention was obviously of a spasmodic and not of a paralytic character, and disappeared after a short time. During the last week of his life a new and peculiar train of symptoms appeared—viz., evidences of respiratory distress—he got cough, with accumulation of mucus in the larger bronchi, which he had extreme difficulty and eventual inability of expectorating. On analysing now the phenomena of respiration, the real state of things (afterwards verified by *post mortem* observation) became apparent; respiration was seen to be almost wholly performed by the diaphragm, and it was clear that we had to deal with a case of complete inaction of the intercostal muscles—the atrophy having, in fact, eventually invaded these muscles. In this condition he died of gradually increasing apnœa.

On making a *post mortem* examination, I found a complete bilateral dislocation forwards of the fifth from the sixth cervical vertebra. I employ the term "complete," it should be remembered, as applying to the condition and amount of displacement of the articular processes—not of the vertebral bodies, which remain partially in contact, the vertebral canal not being wholly obliterated. These processes have entirely slipped from one another, and escaped from contact of their articular surfaces, those of the upper being immovably hitched (into the vertebral notch) in front of those of the lower of the displaced vertebræ. In fact, in making the ordinary section of the soft parts posteriorly, and clearing off the muscles from the gutters on each side of the vertebral spinous processes, one of the first objects which attracted attention was the smooth white cartilage of the superior articular processes of the sixth vertebra, naked and exposed to view. The displacement will be observed to be most remarkably symmetrical, the bifid spinous process of the fifth vertebra just forking that of the vertebra beneath. There is slight bruising (from pressure) of the upper and anterior edge of the body of

the sixth vertebra; and a very minute fragment has been detached from this part and remains adherent to the anterior common ligament—not sufficient, however, to warrant the title of a fracture. There exists some slight consolidation of the osseous tissue of the body of the sixth cervical vertebra, due most probably to some degree of consecutive chronic osteitis. The anterior and posterior common ligaments are both intact and unruptured. As to the condition of the spinal canal, it is suddenly narrowed to almost exactly half its normal calibre, the point of greatest constriction occurring (as is always the case) between the body of the vertebra below, and the arch of the vertebra above, the site of displacement. The spinal cord was not examined microscopically; to the naked eye it presented a slight and very circumscribed constriction at the situation of narrowing of the canal; and on section at this point, there was seen a small greyish spot, about a line in diameter, in the substance of the anterior column. The muscles of the upper extremities, in the atrophied regions, appeared—wherever examined—to be almost wholly reduced to mere sheaves of connective tissue, with longitudinal rows of fat globules, and occasional muscular fibres, which latter—whenever met with—did not present evidences of fatty degeneration. The same condition was observed in the case of the intercostal muscles, although perhaps in a more advanced degree.

I think this case deserving of record, on account, firstly, of the rarity of injuries presenting in so marked a degree, and in so unequivocal a manner, the characters of uncomplicated dislocation of the spine; and secondly, because of the extremely rapid and acute supervention and progress of the muscular atrophy which occurred as its result. Cases have been recorded, I am aware, of “Cruveilhier’s disease” resulting from injury, and I have myself seen one instance at least of the kind; but such cases have generally the chronic and slowly progressive course of the typical disease. I do not know of any other case presenting the peculiarities, in this respect, which I have alluded to as occurring in the present instance.—*December 8, 1877.*

Cerebral Embolisms.—DR. J. W. MOORE said: A woman, aged fifty-four, died in the Meath Hospital yesterday morning after an illness of ten weeks. With the exception of slight indigestion she had enjoyed good health up to the 1st of October last. On the afternoon of that day she came home as well as usual; but in the middle of the night she was seized with cramps and vomiting. She got up, but fell on the floor in a fit. A second fit occurred the following night, and she was found to be paralysed. During the fit the left side of the mouth worked, but not the right. She was admitted to hospital the following day. She was semi-comatose and sleeping heavily. When roused she complained of severe pain in the head and burning heat in the stomach. We learned from her

sister that she had had rheumatic fever twenty-seven years before. She was yawning; her pupils were contracted, and there was complete loss of sensation and of motion on the left side of the face. The mouth was drawn towards the right. The tongue was protruded towards the left; there was left hemiplegia; the heart was weak, but its action was quite regular, and no murmur could be detected. There were bronchial râles in the larger tubes, but no other evidence of thoracic complication. Next day there was marked rigidity in the left arm, and she yawned frequently. No return of sensation in the arm could be detected, but there was slight sensation in the lower extremities, and on this day she was able, to some extent, to draw up her leg. During the following few days she progressed favourably, but after a time her mental power gradually lessened, until she ultimately became insane. We noticed from time to time that the left side of the face was permanently flushed. Her pupils were persistently contracted, and she gradually sank. There had been marked incontinence of urine for some time previous to her death. Unfortunately the heart was not examined after death, but on removing the brain we found the posterior cerebral lobes in a state of very advanced white softening. This was much better marked on the right than on the left side. The arteries at the base of the brain are extensively atheromatous, and in at least two situations in the right middle cerebral artery distinct marks of embolism can be detected. Probably the attack of rheumatic fever had been followed by endocardial and valvular disease; but certainly when she was admitted into the hospital no physical signs of heart disease existed.—*December 15, 1877.*

COPAIBA AND CUBEBS IN THE TREATMENT OF CROUP.

DR. OLDOINI reports five cases of croup cured by these two agents in the cases of children under four years of age during a late epidemic in Spezzia. Every two hours a dessertspoonful of the following syrup was given:—

Bals. copaib. ℥ss.

Pulv. acaciæ ℥v.

Aq. menth. pip. ℥iij. M.

Besides this, he recommends every two hours a tablespoonful of a mixture containing four drachms of freshly-powdered cubebs in ten ounces of syrup. For children much under four years of age the dose can be proportionately diminished. The cases in which he found the good effects of these medicaments were very severe; in all there was intense fever, engorgement of the submaxillary glands, loss of voice, and extreme dyspnœa.—*Annali Universali di Medicina.*

CLINICAL RECORDS.

Notes of three Cases in which the Skull was Trephined for Fracture, occurring in the Surgical Practice of Mercer's Hospital. Under the care of Mr. E. STAMER O'GRADY, Surgeon to the Hospital; Member of the Surgical Court of Examiners, Royal College of Surgeons in Ireland.

INJURIES of the head necessitating instrumental interference are always fraught with the utmost interest to the practising surgeon. Of the following cases,^a submitted without note or comment, the first two are striking examples of the well-known difficulties which occasionally occur in correctly measuring in the first instance the true nature and severity of these accidents; they illustrate too how readily the gravest forms of head injury may, by the total absence of symptoms, be overlooked, and hence the need for thorough and cautious examination under all circumstances (specially when dealing with patients in a condition of intoxication) where fractured cranium could possibly occur. It is open to question whether if the man, the subject of the second case, had come under treatment earlier the result might not with him also have been favourable. I have to thank my hospital colleagues for their courteous co-operation and assistance in the treatment of all the cases.

CASE I.—*Scalp Wound associated with Depressed Fracture of Skull; Trephining; Recovery.*—T. N. (No. 265), a labouring boy fifteen years old, came to the hospital on the afternoon of the 31st March, 1868, to have a cut on his head dressed. This had occurred, he said, by his falling down on the previous day “head-over-heels” into a saw pit, his head striking against a pointed stone lying at the bottom. At the time he was not stunned nor much hurt. There was a scalp wound one and a half inch long on the left side, running from the locality of the posterior superior angle of the parietal in a direction downwards and forwards. Digital examination, which was reluctantly permitted, the patient regarding the wound as insignificant, found at the bottom of it a depression in the bone almond-like in size and shape. The lad, who had walked into the hospital by himself, was perfectly conscious, free from pain, and, save a pulse of 112, had no indication whatever of any departure from health; he said he felt quite well, and it was with considerable difficulty he allowed himself to be retained in the house till his friends could be communicated with. He was placed in a quiet ward, with cold to the head, and some purgative medicine given; after this acted a few grains of true James's powder were directed to be taken.

^a The accompanying lithographs are taken direct from the preparations. The delineations Nos. 1, 2, 3 on the Plate indicate the portions of bone removed respectively in the cases similarly numbered.

FIG. 1.



FIG. 2



FIG. 3.



every three hours. For seven days all proceeded in a most satisfactory manner; the pulse came down steadily to 80; the wound granulated, and was healing nicely, the patient appearing to be, and saying he felt himself in excellent health.

At visit on the morning of the 8th day subsequent to admission something suspicious-looking in the general aspect caught attention; the lad was heavier, and daller far than usual; he was moody and silent, with a slightly contracted brow, and frequent nervous movements of the fingers; ten grains of calomel followed by a castor oil draught were given, and in due course acted naturally. During the day no noteworthy change was observable till about half past five o'clock, when he began to get heavier and more stupid; two hours later he had become nearly insensible, but when told in loud tones to open his eyes or put out his tongue made slight indications of attempting to do so. The pulse remained unchanged from its standard (80) of the past few days. The pupils symmetrical, and acted, but sluggishly so, to light; marked twitching of the fingers of the left hand, together with distinct spasmodic jerkings at the right upper and lower extremities were now present; this condition was most marked in the arm; muscles of the face and neck not affected. A brisk turpentine and assafoetida enema failing to effect any change, it was determined to postpone operating no longer. After shaving the locality the injured bone-surface was fully exposed by opening up the wound and crossing it at right angles with a second incision; in addition to the large depressed piece the bone was seen to be extensively fissured and comminuted; four applications of the trephine enabled all necessary portions to be removed.^a The large piece of the internal table, shown in centre of Fig. No. 1, was much depressed, and deeply indented the dura mater; one of the larger pieces cut by the trephine has unfortunately got lost, and is not represented in the lithograph. Beyond the indentation mentioned, the exposed surface of dura mater presented nothing special in its appearance.

After the scalp flaps were replaced they were lightly supported by strips of adhesive plaster, and an ice bladder applied; one grain of calomel, with two grains of true James's powder were prescribed, to be taken every third hour. It was noted at the termination of the operation, which was done without chloroform, that a considerable amount of consciousness had been already regained; the spasmodic phenomena had also ceased, and it may be mentioned now that these did not again occur.

^a In the removal of depressed and impacted bone in this and the third case a form of forceps, bought by the writer in America, proved of signal service; its jaws are bent at a double right angle; this part of the instrument when viewed laterally resembling a step of a flight of stairs. The lower blade projects somewhat beyond the upper. The great advantage of this extractor is its efficiency in working in a limited space, and capability of effecting removal of depressed bone without further dipping in the distal end as the proximal is being worked on.

On the next morning the patient was reported to have slept most of the night; he had become quite conscious, felt well and free from pain. Pulse 98.

On the second morning there was some œdema, equally well marked, on both sides of the eyelids; the bowels had acted freely. The interval between the powders was directed to be doubled. For some hours during this afternoon and evening much annoyance was caused by extreme acuteness of hearing; this passed away during the night. The patient had been kept confined to bed in the recumbent position, and limited to simple though nutritious dietary.

The progress of the case to recovery continued to be rapid and satisfactory. The mercury was dispensed with after three days. In less than a week the major part of the wound had healed and the remainder was granulating healthily. The edges were kept carefully supported and drawn towards one another by adhesive straps; over these, and corresponding to the opening in the cranium, soft graduated compresses of lint, suitably secured by head bandage, opposed the apparent tendency of the brain mass to here protrude. The patient—now able to be up and about—seemed in every respect in excellent health. The wound was virtually healed in a couple of weeks, though, owing to the escape of some minute scales of bone, cicatrisation was not absolutely complete till the end of the fourth week. Exactly one month from the date of the operation T. N. was discharged from the hospital. Where the bone was deficient the pulsation of the brain mass could be seen, strongly heaving, and prominently raising the integuments, but no sense of inconvenience or annoyance was caused thereby to the patient, who stated that, to his own perceptions, his condition was in no way different to what it formerly was.

It can be affirmed that the sequel of this case continued to be in every way most satisfactory. The subject of it got employed in a mercantile house as a species of under-clerk, having to act also as a messenger and collector of accounts. At intervals he occasionally came to report himself at the hospital, the last time not being many weeks since. He has remained in excellent health and free from trouble of any sort; his only cause of complaint being that he gets drunk too easily, and when intoxicated he has a tendency to be quarrelsome and “wicked”—a condition quite the opposite of his tendencies when sober. This susceptibility to the influence of alcoholic liquors tends rather to increase than otherwise. The volume of the brain pulsation has lessened with time, but still continues to remain well marked, causing, however, as stated, no inconvenience whatever to the patient himself, who has abandoned wearing any form of protective pad or shield over the deficiency in his cranium.

CASE II.—*Scalp Wounds; Fracture of Skull; Trephining; Death.*—T. B. (No. 1,275), aged twenty-six, whilst intoxicated, fell off the driving seat of a cab, his head coming in contact with the tire of the revolving wheel, the man then tumbled heavily and helplessly to the ground—the right side of his face and neck striking it first. At the time, beyond a wound on his forehead and momentary concussion, he was stated to have been but little inconvenienced by the accident; and at the institution where he was brought immediately after the occurrence the injuries were pronounced to be insignificant. Five days later it was observed by himself and his friends that the right side of his face was quite blank; he could not close the eyelids; there was also much pain in his throat and difficulty of swallowing. Two days further on he applied as an extern at Mercer's Hospital; though then better of the paralysis, he could not close the eyes nor distend the cheeks by blowing; the masseters and nuchal muscles were quite rigid, and there was tenderness over the right mastoid region. The tongue, when extruded, inclined to the left, but a little investigation showed clearly that this obliquity was entirely the result of his being able to open the jaw wider on the left than on the right side. The pupils of both eyes were natural, and answered normally to light. Pulse 98. On removing the dressing from his head a quantity of thin ill-smelling pus streamed down. The wound was three inches long and extended perpendicularly up the forehead from the outer extremity of the right eyebrow; its surface was mostly covered with healthy-looking granulations. By the probe the fact of extensive bone denudation was made out, but nothing further was ascertained, the patient being exceedingly intolerant of examination. He was perfectly conscious; made light of his case, quoting the opinion to this effect given him by the gentleman who had previously seen him. Though offered admission he would not then come into the hospital; however, yielding to the solicitations of his friends, he did so the next evening (eight days after the accident). His general condition remained much the same as that described—the facial paralysis rather less marked, but the neck stiffer, the muscles of the right side being tense and contracted, thus dipping the head slightly over towards the right shoulder. Large and frequently-renewed linseed-meal cataplasms were directed to be kept constantly applied to the regions of the wound and affected muscles, and a pill containing one-sixth of a grain of extract of belladonna, with two grains of quinine, to be taken every second hour.

At visit next morning, though a fair night had been spent, the general appearance looked bad, and indicative of mischief. Pulse 105; no change in condition of pupils; the voice had become notably altered, being thick and husky; patient remains quite conscious, and is able to swallow the pills, which he takes regularly; fluid nutriment has also been fairly used. The wound was now dry, and free from discharge;

a director passed under the granulations opened them up, when finger exploration found, at its upper extremity, an irregular surface of bone, not quite so large as a florin, to be bare of periosteum, and roughened to the touch. Traversing this was a well-marked depressed indentation, nearly an inch long, and running in the direction of the external wound. The patient and his friends declined to permit present operation. A turpentine enema was given, and acted freely; the pills also were continued. During the day matters steadily got worse, occasional spasmodic jerking of the right arm and lower extremity occurring. Towards evening there was a well-marked convulsive seizure; this was preceded by a curious and oft-repeated tendency to turn round and round in the bed, the patient rotating from right towards left several times successively in a way which those in the ward called whirling. The pulse had run up to 130. Consciousness remained unimpaired, and the patient now eagerly besought surgical interference. At 9 30 p.m. (entering on the tenth "day" after the accident) he was placed under chloroform, and a short transverse cut made at the upper extremity of the wound; this enabled a triangular flap of the scalp tissues to be raised, and the damaged bone exposed. A large-sized trephine was used, the crown clearing the fractured region. As the instrument deepened its groove a little, the external table, much crushed and damaged, came away in two pieces. The internal, which was splintered and comminuted, was subsequently, as it was cut through, extracted in bits (*vide* Fig. 2). The surface of the dura mater exposed by removal of the disc of bone looked quite natural. The wound was lightly dressed, and cold applied. Two drachms of mercurial ointment were rubbed into the thighs. When seen again at midnight, the patient, notwithstanding he had not slept, expressed himself as feeling comfortable and relieved; stomach somewhat disposed to be sick; an opiate and nutritive enema given. Though much sleep was not obtained, the night was, on the whole, spent quietly. There were, however, occasional but not severe convulsive twitches. Enema was once repeated; urine was passed freely at regular intervals. At morning visit the general aspect was found to be improved. Pulse 108; pupils natural; wound discharging freely a thin sanious fluid; the cold application to the head complained of; French cotton wrung out of hot water, treated with tr. opii, and covered with oil silk, was substituted for it. This dressing was found to be agreeable and grateful. Further use of the mercurial ointment frictions was objected to. In consultation it was agreed to give one-third of a grain of extract of belladonna, with twelve grains of bromide of potash in solution, every second hour. Early in the afternoon the face became flushed, and a rise in temperature to 104° F. was noted.^a The resumption of iced applications to the head was now

^a Registration of the temperature was kept at regular intervals. Save on this one occasion, no higher than 101·5° was reached.

permitted. About 6 p.m. there was a very severe convulsive seizure, attended with frothing at the mouth; this lasted some minutes. Shortly after its termination the temperature was 101° F. Subsequent to this attack much annoyance was caused by unceasing efforts to expectorate a thick and viscid mucus. The progress of the case from this on continued to be steadily downwards. Convulsive attacks occurred with increasing frequency and severity, assuming, so far as the right side of the face and neck was concerned, a suggestively tetanoid character, whilst the left side remained to the last quite free from such trouble. The power of swallowing became impaired, mainly on account of the increasing worry and annoyance caused by the viscid mucus already alluded to, and which painful trials were constantly being made to get rid of by attempted expectoration, aided by efforts to pick it out of the fauces with his fingers. It was sought to sustain strength by nutritive enemata, given at regular intervals; this was systematically carried out, and with benefit; but the patient gradually ran down, retaining consciousness to the last, till he expired at 8 20 on the ensuing morning, being the eleventh day since the occurrence of the accident, and just thirty-five hours after the operation. Unfortunately no *post mortem* could be obtained.

CASE III.—*Scalp Wound, with badly-depressed Fracture of Skull; Laceration of the Membranes; Loss of Brain Substance; Trephining; Paralysis of Right Arm; Hernia Cerebri; Complete Recovery.*—M. L. (No. 2,720), nine years of age, was struck on the head by the rim of a geranium-pot, as it fell from the sill of a first floor window. There was said to have been smart hæmorrhage at the time, also “a fit,” closely followed by a second. Her parents, observing the child to be becoming very drowsy, brought her to hospital, then some hours subsequent to the accident. When seen at 10 30 p.m., a few minutes after admission, she was markedly soporose, though capable of being somewhat roused so far as to show impatience at disturbance. The pupils were natural, and responded to light; the right upper and lower extremities were occasionally jerked by convulsive spasms. The wound, some two inches long, was placed over the posterior superior portion of the left parietal; it was recognisable by both touch and sight that a considerable piece of bone was depressed. Brain substance extruded through the opening. The dura mater in the neighbourhood was extensively separated all round, as evidenced by the free mobility of a probe introduced into the skull through the fractured opening. The parents at once consenting to the operation, it was proceeded with, under chloroform, without delay. Owing to the neglected state of the scalp from filth and extensive disease, the shaving of the head, which was done in a masterly way by Mr. Donnelly—then one of the resident staff of the hospital—was a task exceptionally difficult and repugnant. As the most ready way of exposing the fracture, the existing wound was enlarged slightly forwards,

and a second incision made, running downwards from its centre. Free hæmorrhage, which, during the subsequent steps of the operation, required (as the little scalp vessel could not otherwise be secured) to be controlled by digital pressure, occurred from the anterior angle of the prolonged cut. Two triangular flaps thus formed were raised, bringing the region of the fracture fully into view, and showing that a piece of bone considerably over an inch long, in an antero-posterior direction, was depressed—the hinder extremity, driven deeply in, perforated the meninges and lay embedded in the brain substance which oozed out alongside it. Some small, more or less impacted, bits of bone were around the larger fragment. It being impracticable to stir the depressed bone with forceps, the trephine was applied, covering in the part immediately in front of the large depressed piece. In order to economise the amount of bone necessary to remove, the instrument was successfully worked without the use of the centre pin, being rotated, supported against the left thumb nail till the teeth had made a bed. As the disc was elevated, there came away with it the large depressed piece, bent downwards at an angle, but still its internal table remaining in unbroken continuity with that of the disc, thus constituting a single piece of the whole, as shown in the upper delineation of Fig. No. 3. In the handling of this depressed portion of bone the forceps alluded to in the foot-note to the first case was found most useful. The lower delineation represents the broken and fissured piece, removed, in similar manner, by the trephine from the posterior region of the fracture. Several jagged and projecting angles and spiculæ along the margins intervening between the two trephined extremities of the fracture also required removal, and were nipped off by a small convex bone forceps. Some of these, as well as loose fragments which were preserved, are shown in the body of the figure. Through the lacerated dura mater brain matter kept slowly extruding. The child vomited freely during the operation. At once on its conclusion she fell asleep, and continued so, notwithstanding that in consequence of the troublesome hæmorrhage from the anterior angle of the incision digital pressure had to be maintained there for over an hour ere the wound could be dressed. When the bleeding ceased the flaps were laid down and supported by long broad straps of plaster, so as to maintain a uniform pressure. A small pad of lint, similarly retained, was placed pressing on the site of the bleeding vessel. Iced water-dressings were then applied. Next morning it was reported that the child had a capital night, sleeping nearly right through it—only on one occasion towards early morning being weak, when an ounce of wine was given. She was lively, in good humour, and drew attention to a “queer” feeling in her right arm. This apparently was not of a painful or unpleasant character. On examination there was found to be complete paralysis of motion of the entire right upper extremity. The sense of feeling was also blunted.

Two grains of the true James's powder were directed to be given every second hour, together with a moderate amount of fluid nourishment.

Nothing could be more pleasing than the girl's progress till four nights after the operation, when she had a "fit," during which there was observed convergent strabismus, twitching of the right side of the face, together with spasmodic jerking of both extremities on the right side—the motion of the arm being of a peculiar pump-handle-like character. Consciousness was said not to have been affected. At visit the following morning the child was herself again; it was noted that she could now partially close the fingers and feebly grasp with the paralysed hand. Three grains of calomel were given, followed by a dose of castor oil. In the evening there was a repetition of the "fit," characterised by the same phenomena as the seizure of the preceding night. Some further extrusion of brain matter had occurred. The medicine given in the morning had not acted, though subsequently aided by a turpentine enema. To get half a drop of croton oil with two grains of calomel. Bread and water poultice to head. The bowels were thoroughly moved, and there was no further recurrence of the nervous disturbance.

The originally applied adhesive straps, now loosened by the poultice, were removed for the first time on the sixth morning after the operation. Part of the wound was occupied by a large "*hernia cerebri*," its bulk exceeding that of a full-sized walnut. Gentle but equable and uniform pressure was made by straps of plaster applied well round the head, so as to get a purchase and slide the flaps over to meet and cover, as far as possible, the brain protrusion, between which and the plaster were inserted graduated compresses of soft lint, the whole being further supported by a suitable *caput bandage*. This dressing was renewed from day to day as indicated, and in ten days more the "*hernia*" had become reduced below the level of the bone, with the wound healing in apace. The child was going about daily, regaining power over her paralysed limb. She continued to do well; the wound steadily and rapidly cicatrised; its perfect consolidation, however, being delayed by the exfoliation of a couple of small scales of bone. After a stay of five weeks the girl was discharged from hospital. The persistently diseased condition of the scalp had made the dressing and management of the case matters of some difficulty throughout, and for the completion of the cure of this state she continued to attend occasionally as an out-patient. Save for this particular, she was, at the time of her discharge (and has continued to remain so), in every respect well. Full power over the arm had been regained prior to leaving the hospital. Through the large opening in the cranium the pulsatile action of the brain was very manifest, but there was no apparent further tendency to formation of "*hernia*."

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, January 26, 1878.

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	
Dublin, -	314,666	717	727	8	35	7	4	21	16	7	30.1
Belfast, -	182,082	554	404	—	7	9	1	3	15	11	29.0
Cork, -	91,965	206	254	—	21	—	1	3	5	4	35.8
Limerick, -	44,209	91	118	—	5	3	—	—	2	1	34.5
Derry, -	30,884	71	34	—	—	1	—	—	1	2	14.3
Waterford, -	30,626	51	122	—	33	—	—	—	8	1	52.0
Galway, -	19,692	23	34	—	—	—	—	—	3	—	22.5
Sligo, -	17,285	23	35	—	—	—	—	1	1	—	26.3

Remarks.

The mortality was excessive in Waterford, extremely high in Cork and Limerick, very high in Dublin and Belfast, high in Sligo, and low in Galway and Derry. In London the death-rate was 27.0 per 1,000 of the population annually, in Edinburgh 27.2, and in Glasgow 27.2. Omitting the deaths of persons admitted into public institutions from localities outside the registration district, the death-rate in Dublin was 29.2 per 1,000. The rate within the city itself was 30.9 per 1,000. The deaths from zymotics numbered 127, compared with an average of 151 in the corresponding period of the previous ten years. The number of deaths from zymotics in the preceding four weeks was 137. Whooping-cough and scarlatina were more fatal than in the previous period; but measles, fever, and diphtheria showed a decline. Of the 16 deaths from fever, typhus caused 3, typhoid 9, and simple (?) continued fever 4. It would be far better to classify these last as due to "Continued Fever of Undetermined Type." Of the 8 victims of small-pox, at least 6 were *unvaccinated*. Three of them were men aged 24, 33, and 45 respectively. Measles was less fatal in Cork, but prevailed as a destructive epidemic in Waterford, where 27.5 per cent. of the total mortality was due to it.

The epidemic of this disease in London declined during this period—the weekly number of deaths being 104, 97, 77, and 63, or 341 in all, against 379 in the previous four weeks. Small-pox caused 146 deaths in London, being an increase of 30 compared with the preceding period. In Dublin respiratory diseases were not quite so fatal as on the average in the previous ten years. They caused 198 deaths (average = 200), including 144 from bronchitis (average = 149·7), and 39 from pneumonia (average = 20·9). This comparatively favourable result is explained by the prevalence of mild weather during greater part of the four weeks, the mean temperature of which in Dublin city was as high as 43·3°.

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of January, 1878.

Mean Height of Barometer,	-	-	-	30·153 inches.
Maximal Height of Barometer (on 31st at 10 30 p.m.),				30·707 „
Minimal Height of Barometer (on 7th at 3 p.m.),	-	-	-	29·510 „
Mean Dry-bulb Temperature,	-	-	-	42·6°
Mean Wet-bulb Temperature,	-	-	-	40·9°
Mean Dew-point Temperature,	-	-	-	38·8°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·237 inch.
Mean Humidity,	-	-	-	86·0 per cent.
Highest Temperature in Shade (on 21st),	-	-	-	56·7°
Lowest Temperature in Shade (on 31st),	-	-	-	28·0°
Lowest Temperature on Grass (Radiation) (on 11th)				22·3°
Mean Amount of Cloud,	-	-	-	71·8 per cent.
Rainfall (on 20 days),	-	-	-	1·557 inches.
General Direction of Wind,	-	-	-	W. and S.W.

Remarks.

The mean atmospherical pressure was considerably above the average; but the centre of highest barometer usually lay to the S.E. or S. of Dublin, and consequently gradients for S.W. to W. winds preponderated, causing open weather with an excess of moisture and cloudiness. During the first few days the weather was mild, damp, and overcast. In the interval between the 6th and 10th a cyclone travelled in a S.S.E. direction across great Britain and France. In the wake of this disturbance a rapid rise of the barometer took place, resulting in the formation of a large anticyclone and the descent of a polar air-current over the whole of Western Europe. The weather became drier and colder, so that on the morning of the 11th there were 10 degs. of frost on the grass in Dublin—where, owing to a calm, a dense smoke-fog prevailed for some hours. The anticyclone became established over France and Spain on the 12th, from which day to the 24th pressure was higher in those

countries than in the United Kingdom. In fact, several depressions, travelling eastwards across Northern Europe during this period, caused steep gradients for W. winds in Ireland and Great Britain, where mild and rather stormy weather prevailed. In France it was, meanwhile, very cold—the thermometer standing at 19° in Lyons on the morning of the 13th. After the 20th very unsettled weather with extreme contrasts of temperature was experienced. During *the night* of that day the thermometer rose to 55.6° in Dublin. Two days later temperature decreased, and sleet and snow fell in Dublin, with wild and wintry weather. Violent N.W. winds swept over the country on the 24th and 25th, with falls of hail and snow. At the end of the month another anticyclone, accompanied by clear cold weather, came in over Western Europe. Snow or hail fell on the 7th, 23rd, 24th, and 25th. A lunar halo appeared on the 13th, a solar halo on the 18th, and a lunar corona on the 17th. Fog were noted on no fewer than nine days. The rainfall was below the average amount.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

CONGENITAL DISPLACEMENT OF BOTH CRYSTALLINE LENSES.

At a meeting of the Medical Society of London, held on Monday, Jan. 14th, 1878, Mr. Wordsworth exhibited six persons belonging to one family who are all affected by congenital displacement of both crystalline lenses. They consist of Mrs. H., her two sons, and their three children. A third son of Mrs. H. (who is now in New Zealand) was examined many years since, and his case described in the 1st Vol. of the "Royal London Ophthalmic Hospital Reports" by Mr. Dixon. In addition Mrs. H. states that her own father, his youngest brother, and her grandfather were all similarly affected. If this be so, there is a series of *ten* cases occurring in *five* successive generations. They all complain of being short-sighted, and on close examination by oblique illumination, and when viewed by the ophthalmoscope, the crystalline lens in each eye is seen to be displaced, and its edge visible in the pupil. In distant vision they look through that part of the pupil in which the crystalline is absent, and for near sight through the crystalline itself. The family recognised the defect in all who are subject to it, and in those they considered exempt the examination confirmed their opinion. The annexed statement, made by Dr. Morton, Clinical Assistant at the Moorfields Hospital, fully shows the particulars of these very interesting cases:—Mrs. H., aged fifty-nine. Displacement—R. E. upwards and inwards; L. E. do. ^{do} Irises light-brown, lenses becoming opaque. V.=R. E. and

L. E., $\frac{20}{200}$ scarcely and J. 1 at 8"; after atropine, with $\frac{1}{4}=\frac{20}{50}$, with $\frac{1}{2}=J. 1$ at 6". George H., aged thirty-seven. Displacement—R. E. inwards; L. E. inwards and slightly downwards. Irides grey-blue. V.=R. E., $\frac{20}{200}$ nearly and J. 19, with $\frac{1}{20}=\frac{20}{70}$, with $\frac{1}{3}=J. 1$ at 6"; L. E., $\frac{20}{100}$ and J. 1 at $1\frac{1}{2}''$, with $\frac{1}{14}=\frac{20}{30}$, and with $\frac{1}{3}=J. 1$ at 5". William H., aged thirty-five. Displacement—R. E. upwards and inwards; L. E. inwards. Irides brown. V.=R. E., $\frac{20}{200}$ and J. 1 at 2'', with $-\frac{1}{2}=\frac{20}{100}$, with $\frac{1}{3\frac{1}{2}}=\frac{20}{90}$ and J. 1 at 8"; L. E. not $\frac{20}{200}$ and J. 1 at 2'', with $-\frac{1}{2}=\frac{20}{100}$, with $\frac{1}{3\frac{1}{2}}=\frac{20}{90}$ and J. 1 at $5\frac{1}{2}''$. Maud H., aged ten. Displacement—R. E. inwards; L. E. do. Irides grey. V.=R. E., L. E., $\frac{20}{200}$ and J. 1 at 2"; after atropine, with $\frac{1}{12}=\frac{20}{100}$, with $\frac{1}{2}=J. 1$ at 4". Alfred H., aged five. Displacement—R. E. upwards; L. E. upwards and inwards. Irides grey. Vision not proved. George H., aged seven. Displacement—R. E. upwards and outwards; L. E. upwards and inwards. Irides brown. Vision not proved.

EFFECTS OF SECTION OF THE SPINAL CORD UPON ANIMAL TEMPERATURE.

PROFESSOR H. C. WOOD, junr., of Philadelphia, was some time since granted the sum of £100 by the Smithsonian Institute to enable him to prosecute with thoroughness and success a series of experiments relative to the effects of artificial lesions of the various parts of the nervous system upon the evolution and production of animal heat. As concerns the effects of the section of the cord, Professor Wood, after a number of painstaking and laborious experiments, has been able to reach the following conclusions, which are given in the *N. Y. Med. Rec.* of Jan. 19:—At a low external temperature, after section of the cord, there is increased evolution with diminished production of heat; at a high external temperature, both the production and evolution of heat are diminished. After section of the cord the animal dies in winter of cold, and in summer lives much longer on account of the high external heat. It will be at once noted that this fact has a very important application to narcotic poisoning, to collapse, as seen after exhaustion by heat, &c., and to thermic fever; the hot bath being the best treatment for collapse and narcotic poison, and the cold bath for thermic fever. [Collapse from heat and thermic fever are usually confounded. In collapse the internal temperature is below normal, while in thermic fever it is above.] Professor Wood has also discovered the fact that the instant fall of temperature, after section of the cords, is due to vaso-motor paralysis, producing paralysis of the arterioles at the surface, thus obliterating the superficial cold layer of the body, and removing the only barrier to the animal's internal heat. Dr. Wood has thus shown that temperature is no guide to the amount of heat given off in such cases, and that fever simply reduces itself to a question of the difference in the relation of the heat-

producing and heat-evolving powers; that the heat-evolving power is no longer compensatory to the heat-producing when the cord is cut. The experiments of Owsjannikow, which located the governing vaso-motor centre of the brain in a space whose upper boundary is one or two millimetres below the corpora quadrigemina, and whose lower boundary is from four to five millimetres above the point of the calamus scriptorius (*Berichte*, &c.; Bd. XXIII.), have received full confirmation at Dr. Wood's hands.

NEW PREPARATIONS AND SCIENTIFIC INVENTIONS.

MR. WILLIAM HAY, of Hull, has kindly sent us a sample of concentrated transparent essence of Jamaica ginger, free from resin. After many trials Mr. Hay at last succeeded in completely eliminating the acrid, somewhat bitter resin of which Jamaica ginger always contains a considerable quantity. His new preparation, therefore, possesses a great advantage over the officinal and all other essences of ginger which contain this resin, and so not only form turbid solutions with all aqueous mixtures, but are also unpleasant and nauseous to the taste. In other essences of ginger all the aromatic qualities and fine flavour of the essential oil, of which Mr. Hay's preparation is a solution, are in a great measure neutralised by the presence of the same resin.

This new essence will supply a desideratum, and no doubt prove of great use, not only as a dietetic agent, but in all cases where ginger is required for medicinal purposes.

IN MEMORIAM—W. STOKES, M.D.

NOTES.

Page 198.—It should be mentioned that in 1876 the Medical Board of the Meath Hospital presented a marble bust of Dr. Stokes to the Hospital. It was sculptured by Mr. Tenniswoode from Foley's statue, and occupies a niche in the Hall of the Hospital, which is also adorned by busts of Sir Philip Crampton, Mr. W. H. Porter, Mr. Maurice Collis, and Mr. Maurice H. Collis.

Page 200.—“Where, mingling with the wreckful wail,
From low Clontarf's wave-trampled floor,
Comes, booming up the burthened gale,
The angry sandbull's roar.”

These lines are taken—with a slight alteration, the substitution of the word “Where” for “When”—from the beautiful poem, entitled “The Cromlech on Howth,” by Samuel Ferguson, Esq., Q.C., M.R.I.A. The poem is illustrated with illuminations from the Books of Kells and of Durrow, and drawings from nature by Miss Stokes. In a note on the lines quoted, Mr. Ferguson says—“The same idea which led the classical ancients to symbolise a turbulent river under the figure of a bull, has given to the sand-bank forming the bar of the River Liffey, among the Irish, the name of “Tarbh,” or “Tarv,” the Bull, from the bellowing sound of the surf; whence also Clontarf, or the Recess of the Bull, the scene of the defeat of the Danes, A.D. 1014.” More properly, however, Clontarf means “The Meadow of the Bull” (*Cf. Joyce's Irish Names of Places*, Fourth Edition, p. 472).

J. W. M.

In Memoriam.

FLEETWOOD CHURCHILL,

M.D. EDIN., M.D. DUBLIN (HONORIS CAUSA), F.K.Q.C.P., &c.

Obiit Jan: 31, 1878, ANNO ætatis 70.

It is but one short month since the pages of this Journal recorded the death of William Stokes ; it is now our sad duty to record that of one of his contemporaries, Fleetwood Churchill. As Stokes was the leader of the great branch of medical science in which he practised, so Churchill may be spoken of as for many years the leader of obstetrical science in Dublin. Both retired from practice almost together, and both have passed from amongst us about the same time. Fleetwood Churchill died on the 31st of January, at Ardrea Rectory, the residence of his son-in-law, the Rev. Dr. Meade, with whom he had resided since his retirement from practice. He was within a month of reaching the threescore years and ten allotted as the life of man. Dr. Churchill was an Englishman by birth, but early in life settled in Dublin, united by a loving tie to what was afterwards his field of labour and of triumph. Although an Englishman, we must claim Churchill as one of ourselves—one who was a thorough citizen of Dublin in all things which constitute true citizenship, while like all good men he was cosmopolitan in all things which are for the welfare of humanity.

Dr. Churchill graduated in medicine at Edinburgh in 1831 ; in the following year he became a Licentiate of the King and Queen's College of Physicians. He immediately devoted himself to the practice of midwifery, and, in conjunction with the late Dr. Speedy, established the Western Lying-in Hospital. This institution for many years performed valuable services for the poor women in the western portions of the city of Dublin, and we have heard that its loss was much felt when it ceased to afford aid. Soon after its foundation this hospital furnished the materials for some of Dr. Churchill's earliest papers in the *Dublin Journal of Medical Science*. These were followed by others, and even within the last two years he has contributed to our pages. His clinical memoirs

and miscellaneous papers on operations connected with obstetrics are innumerable. In 1838 he published the first of those text-books which have added such world-wide lustre to his name; this was his treatise on "Diseases of Women"—a work which has since passed through six editions. In 1840 he published observations on "Diseases of Pregnancy and Childbed;" in 1841 "Researches on Operative Midwifery." In 1842 appeared Churchill's "Midwifery," which at once took its place as the text-book of the day. In this work the author not only recorded his own experiences, but concentrated all the knowledge known up to that time upon the subject. He was a most industrious compiler of statistics, and the statistical tables used to illustrate his Midwifery were not before approached, and have never been surpassed; six editions of this book have been published. In 1849 the first edition of his work on "Diseases of Children" appeared, and it has now reached its third edition. The circulation of his works has been of the most extended character; they have been re-published in America, and have been translated into many foreign languages, including the rare distinction of being rendered into Chinese.

In 1856 Dr. Churchill was appointed King's Professor of Midwifery in the School of Physic, which office he held until the year 1864. In 1856, and again in 1864, he was President of the Obstetrical Society of Dublin. He occupied the Presidential Chair of the King and Queen's College of Physicians during the years 1867 and 1868. In 1851 the honorary degree of M.D. was conferred upon him by the University of Dublin. In 1874 he was chosen President of the Obstetrical Section of the British Medical Association, but owing to the state of his health he was unable to attend the meeting. From this time his health failed so quickly that in the following year he had to retire from practice. On his retirement from the practice of his profession in 1875, he presented his valuable obstetrical library to the College of Physicians—a gift which the College acknowledged by presenting him with a complimentary address, and the placing in the College Hall of his portrait by Mr. Jones, President, R.H.A.

Dr. Churchill did not confine his attention merely to his profession and the branch of it in which he worked so diligently. He took an active part in all medico-political questions, was an earnest and laborious worker on the committees of his College, and an able and energetic debater in all

questions discussed in the College meetings. He even went much beyond this—he was one of the first pioneers of sanitary reform, having assisted in founding the old “Dublin Sanitary Association” so far back as the year 1850. This Society, being before its day, failed to accomplish its objects; but when sanitary reform at length made its voice heard, Dr. Churchill was again one of the founders of the new “Dublin Sanitary Association”—a body which owes much of its success to his guidance as chairman of its Executive Committee.

Dr. Churchill was for many years an active member of the Council of the Irish Medical Association, and was also a constant attendant at the meetings of the Council of the Royal Medical Benevolent Fund Society of Ireland.

Before his practice became so extensive, Dr. Churchill was a diligent worker at the sciences collateral to medicine; and when the microscope had made but little way as an aid to medical science, he was an earnest microscopist, and one of the earliest members of the Dublin Microscopical Club. He was a lover of books, and an extensive reader of general literature; for some time also he was a working member of the Library Committee of the Royal Dublin Society.

The *Daily Express*, in its obituary remarks on Dr. Churchill, says:—“Among his friends he was known as an ardent supporter of foreign missions, the hospitable friend of missionaries, and better acquainted with the history and work of the Church abroad than almost any of his contemporaries. When the Church Congress was held in Dublin, in 1868, he was selected to read a paper on ‘The American and Canadian Churches; their organisation and practical working.’ That paper struck all who heard it as very remarkable, not only for its research and accuracy, but for the calmness and foresight which it displayed in prospect of the altered conditions then coming on the Church of Ireland. When the Act of Disestablishment was passed, no Irish layman assisted more than Dr. Churchill did towards the arduous work of re-organisation. On this and on the financial arrangements of the Church he contributed some valuable papers and pamphlets, which were largely used and referred to by the Church Representative Body, by whom Dr. Churchill was gladly welcomed into council on these questions. Not merely with pen, but largely and generously with purse also, did he contribute. But those who knew

him most intimately knew best how his Christian modesty of character would shrink from praise in such a matter. His life was an example of an unostentatious, practical Christianity—a living lesson of religion pure and undefiled. Its governing principle was Christian duty, and its closing years exemplified the Psalmist's words—'Mark the perfect man, and behold the upright, for the end of that man is peace.'"

He was a member of the provisional committee for promoting Hospital Sunday in Dublin, and took a deep interest in the success of that movement.

Dr. Churchill was one of those many-sided men who did not confine himself—as is too much the habit with members of our profession, especially the successful ones—to the hard-and-fast lines of his profession. There was no good object which he could aid, in the promotion of which he failed to assist. He was never afraid of being thought to have nothing to do, or to be neglecting his business because he was engaged in works of charity, philanthropy, or religion. The bright face in the sick room, the winning way with children, the sympathy with the joys and sorrows of those who came in contact with him, will for ever make his memory dear to all who knew FLEETWOOD CHURCHILL.

T. W. GRIMSHAW.

THE DUBLIN JOURNAL

OF

MEDICAL SCIENCE.

APRIL 1, 1878.

PART I.

ORIGINAL COMMUNICATIONS.

ART. VIII.—*The Principles of Electro-Therapeutics.*^a By WALTER G. SMITH, M.D., F.K.Q.C.P.I.; Assistant-Physician to the Adelaide Hospital.

LECTURE II.

Synopsis.—Reinstatement of the Galvanic Current in practice. Methods of applying Electricity :—(a) Direction method—(b) Brenner's polar method. Penetration of Electric Currents through the Brain and Spinal Cord. Galvanisation of the Sympathetic. Action of Electricity upon the Motor Apparatus. Applications of Electricity to Diagnosis and Prognosis—(a) Central Cerebral affections—(b) Central Spinal affections—(c) Peripheral Nerve lesions—(d) Myopathic lesions. Modes in which the therapeutical action of Electricity is exerted. Conclusion.

GENTLEMEN,—In the preceding lecture we had under consideration the varieties of currents, the relations between their physical attributes and physiological properties, the methods of developing these currents, and the laws of the diffusion and limitation of electric streams in the human body. In to-day's lecture I propose to instance some modern investigations on the diffusion of electric currents through the central nervous organs, and to review the action of electricity upon the motor apparatus. The remainder of the time will be occupied with the applications of electricity to the important subject of diagnosis and prognosis, concluding with an

^a Two lectures delivered before the King and Queen's College of Physicians in Ireland, on February 4th and 11th, 1878, being the first and second of the Annual Scientific Lectures for 1878.

outline of the scanty store of knowledge we possess on the manner in which the beneficial action of electricity is exerted in disease.

Let me in the first place advert to the causes of the increasing estimation in which the constant galvanic current is now held, and to the methods of procedure in applying electric currents therapeutically.

Within the last few years attention has been chiefly directed to an endeavour to determine with accuracy the particular spheres of action of the galvanic and faradic currents respectively. It is now universally recognised that each form of current has its value under different conditions, although it must be allowed that in the choice of the therapeutic application of either current we have still to follow empirical observations rather than theoretically founded indications.

In consequence of the brilliant results which were obtained with faradic or induction currents, the galvanic current was for a long time neglected and almost forgotten, and Remak did good service, although scantily acknowledged in his lifetime, by his persistent endeavours to reinstate the constant current in practice—a revival which has been pronounced by Niemeyer to be “one of the most valuable advances of modern times.”

The principal reasons for the pre-eminence now accorded to the galvanic current are these:—

1. It possesses a wider range of action than the faradic current—for example, in some cerebral and spinal paralytic cases, and in many peripheral palsies, it will effect what faradic currents cannot accomplish, and in sedative influence it predominates beyond all question. 2. It possesses remarkable powers of penetration and diffusion.

Thus a moderately strong galvanic current applied at great distances from the head and neck will frequently produce its well-known and unmistakable action upon the gustatory and optic nerves.

I may remark in passing that this remote action upon the gustatory nerves, which sometimes persists long after the removal of the electrodes, effectually disposes of the electrolytic explanation of this familiar phenomenon, and shows that the sensation is actually due to the irritation of the specific energy of the nerve. No other agent exercises such a remarkable action upon the organs of special sense as the constant galvanic current.

The principles upon which the methods of faradisation are based

were explained in the first lecture, and there is a common consent of opinion and usage in most of the details of practice of what may be termed Duchenne's polar method. But upon the methods to be followed in galvanisation there has been, and continues to be, free controversy.

Two different plans have been zealously advocated by their respective supporters:—

1. The direction method; or, physiological method, as it is somewhat ambitiously styled.

2. The polar method of Brenner.

According to Remak, Benedikt, Onimus, and others, who favour the direction method, it is of the greatest consequence whether an ascending (centripetal) or a descending current (centrifugal—*i.e.*, + pole nearer to the nerve centre) be employed, and precise and dogmatic instructions are given as to the indications for each. The same current will, it is said, in the one direction soothe and in the other irritate; in the one case contract the vessels, and in the other cause dilatation.

Intimately connected with this question of direction is the difficult one of electrotonus—*i.e.*, the modifications of irritability impressed upon a nerve by the passage of a continuous galvanic current—upon which so many and so contradictory experiments have been published. Enormous physiological and physical^a difficulties combine to entangle this problem, and the best authorities differ on fundamental points.

Experiments instituted upon the exposed and isolated nerves of frogs should be applied only with great reservation to the study of the properties of the uninjured nerves of living man, surrounded as these are by other good conducting tissues. Unsubstantial therapeutical theories upon “the systematic production of catelectrotonus and anelectrotonus,” have been deduced from such experiments, and we are gravely told that the therapeutical effects of the current in rheumatism, neuritis, &c., are probably owing to “catelectrotonus of the vaso-motor system of nerves.”

The discrepancies, moreover, in the results of experiments carried out on the human subject, with the object of checking the physiological experiments, are at present hopelessly irreconcilable,

^a The plan of employing the faradic current as the testing current, while the galvanic current forms the polarising current, is very faulty. The two currents, if they run in the same direction, reinforce each other; if in the contrary direction, they weaken each other.

and the answer to those who, with Cyon, hold that "the science of electrotonus is of the last importance for all who occupy themselves with the application of electricity to therapeutics," is—*not proven*.

Although Brenner's fundamental position is too absolute—viz., that the direction of the current is wholly without importance, since physical considerations on the diffusion of currents, as well as Erb's, Burckhardt's, and Ziemssen's conclusive experiments, refute his *dictum* that it is impossible to give a current a definite direction through human nerves and muscles—yet we are still far from having a satisfactory basis for an electro-therapeutical system built upon the direction of a current.

Brenner's polar method consists essentially in placing one electrode on an indifferent spot (sternum, or patella), while the other electrode is brought into the closest possible relation with the part to be tested, and can, by a commutator, be employed either as an anode (+ pole) or cathode (— pole).

Runge's experiments in 1870 confirm Brenner's propositions in the main, and Erb (no mean authority) adopts the polar method in his practice. Inasmuch as Brenner's statements, although they have been published for some years, do not appear to have attracted in England the attention which the scientific character of their author merits, I will select for reproduction the most important of the conclusions to which his studies on the action of the galvanic current upon the motor nerve fibres of healthy uninjured human subjects have led him:—

(a.) The physiological action of the poles is as different as the chemical. When the poles are placed upon dissimilar spots, the influence of that pole will always predominate which corresponds to the physiologically different and more excitable point—viz., that nearer to the nerve centre.

(b.) The less the difference in the points of application of the electrodes is in regard to excitability, the more nearly identical will be the action of the two poles.

(c.) For these reasons we should place a nerve, which is the subject of electrical investigation and treatment, as closely as possible within the influence of that electrode whose specific action corresponds to the object of the investigation or treatment.

(d.) The closing contraction depends upon the cathode, the opening contraction upon the anode.

(e.) It should be borne in mind that, in the sphere of each single

electrode the actions belonging to it are not exclusively manifested, but that the influence of either electrode is observed, in a less degree, in the sphere of the other electrode.

From a clinical point of view far less importance attaches to the influence of the direction of a current than is often taught, and there is reason to anticipate that the polar method of excitation is destined to gain a wider acceptance, and to prove its claim to be regarded as a rational basis for practice. In the therapeutics of nerve and muscle we commonly employ a third method, which, for distinction, may be termed the bi-polar method, and corresponds to Duchenne's method of faradisation. For example, in the treatment of facial palsy by localised galvanisation, both poles are placed near each other on the face, or one inside the cheek and the other on the face. Already we know a good deal respecting the differential action of the two poles—thanks chiefly to Brenner's careful investigations, and the central fact to remember is this:—

Both with the galvanic and the faradic currents, the action of the negative pole is quantitatively more intense than that of the positive pole, on sensory as well as motor nerves, the conditions being the same for each electrode.

This can be easily demonstrated as follows:—If we simultaneously stimulate two corresponding nerves of a healthy subject, the effect will always be greater in that part to which the cathode or negative pole is applied, whether the positive pole be nearer the centre or the periphery. [Demonstrated on the facial and median nerves.]

Similarly, if two homologous facial muscles be simultaneously irritated, one with the + pole, and the other with the — pole, the contraction and sensation will always be more energetic on the side of the — pole. Alternation of the poles will bring out this result in a striking way.

A practical deduction from this is, that to gain the maximum stimulating effect from a current of minimum strength, the negative pole, of small dimensions, should be applied to the part operated on, while the circuit is completed by the positive pole of larger dimensions. This should be a fundamental rule in the therapeutical application of galvanisation or faradisation to nerves and muscles.

The problem whether it is possible to obtain a direct action of an electric current upon the brain and spinal cord is of great interest; its possibility was long denied, but has now been satisfactorily proved. In the last edition (1872) of his work (*Die*

Electricität in der Medicin), Ziemssen candidly avows that up to the year 1866 he sided with those who explained by reflex action the phenomena evoked by the application of a galvanic current to the brain. But, converted by the remarkable experiments of Erb, in 1867, and of Burckhardt, in 1870, Ziemssen followed up the subject further, and arrived at results confirmatory of theirs.

It has now been demonstrated that during the application of a galvanic current to the head, derived currents, of notable intensity, pass directly through the brain; that the intensity of these currents is greatest in the right line joining the two poles; and that their direction alters in accordance with that of the principal current (Plate I., *f*).

The following arguments will suffice to establish the truth of this conclusion, and to show that the obstacles encountered by a galvanic current on its way to the brain are not so formidable as they were supposed to be:—

1. Between the epidermis and the bone there is only a thin layer of better conducting soft tissues, through which a diversion of the current could occur.

2. Bone, in consequence of its permeation by blood-vessels, is not such an imperfect conductor as was commonly believed—in fact, the cranial bones offer less resistance than the epidermis.

3. The electric stream issuing from the electrodes is broken up into a number of small currents, each of high density, which rush through the blood-vessels that pierce the cranium.

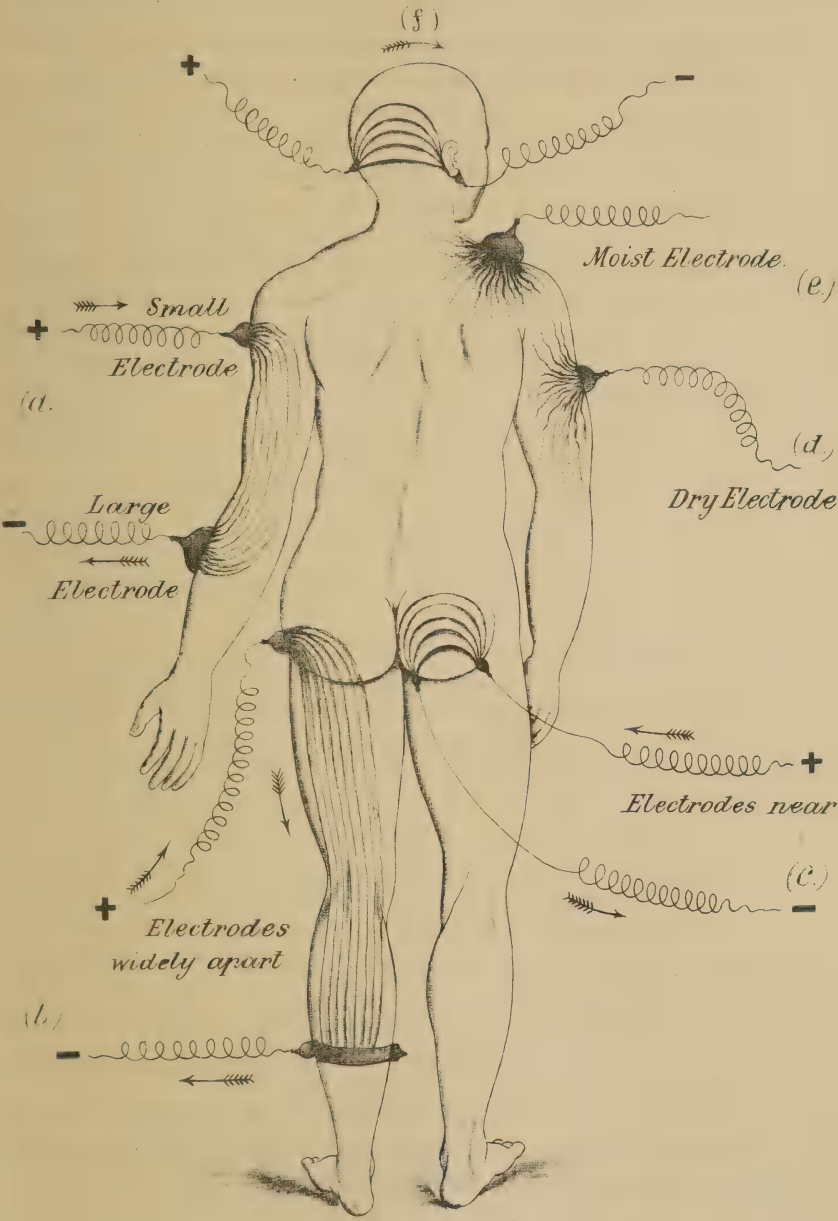
4. The conductibility of the brain is remarkably high in comparison with that of other organs, in consequence of its large proportion of water and its homogeneous structure.

5. Erb's experiments with the galvanoscopic frog upon dead subjects, repeated and controlled by Burckhardt and Ziemssen, show distinctly that both galvanic and induced currents, of an intensity the same as that employed in therapeutics, can be made to pass through the substance of the brain (*Cf.* Ziemssen or Althaus).

6. These arguments and experiments greatly enhance the significance of observations carried out on living man by clinicians and physiologists, and the ridicule cast on Remak's statements recoils upon his objectors.

The phenomena which are observed in consequence of galvanisation of the brain are of two kinds:—

- (a.) Subjective sensations on the part of the organs of special



sense—*e. g.*, luminous appearances, gustatory sensations, buzzing in the ears, &c.

(*b.*) Symptoms of direct irritation of the brain—*e. g.*, giddiness and tendency to somnolence; and, with stronger currents, faintness, convulsions, nausea, and even vomiting.

The striking and familiar fact, that the evidence of galvanic excitation of the retina precedes the symptoms of irritation of the brain, depends upon the superficial position of the ball of the eye, and upon the high conductivity of it and of its surroundings. The bulbus oculi contains 90 per cent. of water, and, as might have been expected, Ziemssen has proved that it is one of the best conductors in the body. This consideration removes all difficulty from the explanation of the very acute excitability of the retina.

A uniform and singular result of direct galvanisation of the head is the giddiness or dizziness already alluded to. In the course of treatment of facial neuralgia and facial palsy by the galvanic current, patients constantly remark it, and I have frequently known them to complain that they felt a tendency to sink off from the chair and fall to the ground. This curious phenomenon has naturally not escaped the attention of physiologists. More than fifty years ago Purkinje made some interesting observations, and the subject has acquired fresh interest by the recent experiments of Fritsch and Hitzig, of Ferrier, and of the numerous inquirers who have been stimulated by their novel investigations. It is not unworthy of recollection that Hitzig's physiological researches took their origin from definite observations made upon man, in whom he noticed involuntary movements of the globe of the eye as the result of directing a galvanic current across the head between the mastoid process and lobe of the ear—*i.e.*, in the auriculo-maxillary fossa.

The dizziness is most readily caused by placing the + electrode on one mastoid process and the — electrode on the other, and anyone who will try this simple experiment on himself with a feeble current—5 to 10 Leclanché cells—can verify the fact that the tendency to fall is usually towards the side upon which the + pole (anode) is placed. On completing the circuit, irritation is strongest at the — pole, with giddiness and oscillation of the head towards the positive pole; on breaking the circuit the converse phenomena happen.

The physical reasons for selecting the mastoid regions are—the thinness of the skin, the absence of hair on these parts, and, above all, the presence of large vessels (posterior meningeal artery

through mastoid foramen: vein connecting lateral sinus with the posterior auricular veins) which greatly facilitate the transmission of the current.

That the disturbance of equilibrium is not merely subjective can further be demonstrated by Immermann's brush. A camel's hair brush was fixed upon the head so as to record the movements of the head and trunk upon a carbonised plate, arranged horizontally above. It was then found that the actual movements of the head were not so great as the subjective sensations appeared to indicate.

The cause of this galvanic dizziness is referred by Ferrier to irritation of the cerebellum; but it is quite possible—nay, even probable, that it may depend upon irritation of the semicircular canals—*i. e.*, of the ampullar nerves.

The remarkable cerebral phenomena already mentioned are observed almost exclusively under the use of the constant galvanic current, and obviously suggest extreme caution in galvanising the head. Dr. Clifford Allbutt has met with one instance of sudden syncope occurring during the application of a gentle faradic current to a case of Bell's palsy (*Brit. & For. Med. Chir. Rev.*, Oct., 1873); this may have been a mere coincidence.

From the foregoing it is plain that we are now, for the first time, in a position to undertake on a reasonable basis the galvanic treatment of central nervous diseases. Although some facts have been already contributed, little is yet definitely known. It remains for clinical experience to furnish the precise indications, and it is greatly to be hoped that the inquiry will be conducted unhampered by theoretical bias or fanciful hypotheses.

If we turn now to the kindred question of the possibility of electrically influencing the spinal cord directly, a few words will suffice to state the arguments which prove that this also is feasible.

1. Although there is a thick layer of superficial soft parts of good conductivity, and we are able to reach the cord from one side only of the vertebral column—conditions differing from those met with in the cranium—yet, on the other hand, we can safely direct along the spine currents of high intensity; and,

2. The vertebral bones offer less resistance than the skull to the passage of a galvanic current, owing to their anatomical conditions, for they are more spongy, contain more water, and there are innumerable lacunæ, and foramina traversed by blood-vessels—all of which circumstances greatly favour the direct penetration of the current.

3. Hence, as in the case of the brain, the electric stream, issuing from the electrodes, is broken up into a number of small currents, each of high density, which flow in along these little fluid channels.

4. The spinal cord, like the brain, is an excellent conductor.

5. The experiments of Erb, Burckhardt, and Ziemssen upon the dead subject gave equally positive results with those obtained on the brain.

6. Experiments on the living man also go to show that an electric current of suitable intensity can penetrate into the vertebral canal. By applying a powerful current, from 24 to 30 Leclanché cells, positive electrode on the upper dorsal vertebræ, and negative electrode on the middle lumbar vertebræ, it is possible, using voltaic alternatives, to produce contractions in the hamstring muscles, while sensations of tingling and heat are awakened in the legs, feet, and toes.

7. Clinical observations—viz., the remarkable and not always transient improvement effected in some spinal affections—*e. g.*, locomotor ataxy, by the galvanic current—point in the same direction, and, in short, there is enough to convince us that it is no longer unjustifiable to speak of directly galvanising the spinal cord.

But here we leave sure ground, and venture on the shifting sands of hypothesis when we speak of galvanisation of the cervical sympathetic (no one except Benedikt pretends to galvanise the thoracic or abdominal sympathetic), a subject upon which much ink has been expended, and little knowledge accumulated—one which tempts imaginative therapeutists, and upon which sober writers still suspend judgment.

On simple physical principles, referred to in the first lecture, independently of the positive experiments of Burckhardt and Ziemssen, it is evident that when an electric current of moderate intensity is directed along the side of the neck beneath the angle of the jaw, derived currents will diffuse themselves through the cervical sympathetic. But from anatomical and physical considerations it is equally plain that it is nothing short of ludicrous to speak of isolated stimulation of the cervical sympathetic cord, as if the current made straight for that particular nerve, just as it would be irrational to deny that among a bundle of conductors one should escape the electric influence.

The facts adduced in support of the doctrine are of the most

inconclusive kind—*e. g.*, the observations upon the condition of the pupil and of the retinal circulation, and upon the cardiac action and blood pressure—and the results obtained by different experimenters are so contradictory as to be devoid of value.

In all the experiments a number of factors intervene whose effect on the circulation and vascular pressure in the brain and other parts may, under certain circumstances, be even more powerful than electrification of the cervical sympathetic itself. We cannot electrically influence the sympathetic in the neck without at the same time influencing the neighbouring nervous apparatus—*viz.*, spinal cord, base of brain, vagus, depressor, and laryngeal nerves, and it is really curious to note how the vagus is tacitly left out of account in the fanciful speculations indulged in on this topic.

The superstructure of pathology and therapeutics, based upon galvanic stimulation of the sympathetic, which has been so impulsively erected by some writers of repute, is like an inverted pyramid balanced upon a tiny point. The wider the generalisations, and the higher the therapeutical aims, the more unsteady the edifice. From an unbiassed survey of the issues in dispute, it seems that, as matters stand at present, the gravest doubts beset the possibility of directly irritating the sympathetic to any purpose, as well as the results to be gained from such a procedure under physiological and pathological conditions.

Numerous additional methodical experiments on living man are still needful before the problem can be solved; meanwhile let us, if we will, speak from clinical experience of the effects of *cervical* galvanisation or *cervical* faradisation, and accept such beneficial results as may be reached, without prejudice to the future explanation of the channels through which these effects are attained.

In relation to the action of electricity upon muscles and motor nerves, which belongs to the most familiar daily applications of electricity to practice, time warns me to be very brief.

Thanks to the converging labours of many workers, light has been freely shed over the chaotic darkness that obscured this subject, and much has been done towards the establishment of a clearer understanding between the teachings of experimental physiology on the one hand, and of electro-therapeutics on the other. The gist of the matter can be simply and concisely stated in a few propositions, as follows:—

1. Healthy muscles and motor nerves respond to electric currents.

2. This response takes place according to definite laws, to be presently adverted to, and which hold good equally for nerve and muscle.

3. Excitation of the motor apparatus results chiefly from variations in intensity of the current, and is proportioned to their extent and suddenness (Du Bois Reymond).

4. Practically, therefore, motor irritation occurs only or mainly at the moments of opening or closing the current—*i. e.*, at the periods of maximum oscillation of the current.

5. Hence, a constant galvanic current must be interrupted in order to cause motor excitation. Induced currents are necessarily interrupted by their mode of genesis.

6. With either form of current, as we have already seen, the stimulus of the $-$ pole is stronger than that of the $+$ pole. This feature is best marked with the galvanic current.

7. Induction currents produce a tonic contraction—*i. e.*, a tetanising of the muscle, which lasts as long as the faradisation, because the shocks succeed each other so rapidly—amounting to many thousands in one minute—that a complete alternation of contraction and relaxation cannot take place. The opening contraction is much the stronger, as was demonstrated in the last lecture.

8. If we experiment with a constant galvanic current on the motor system of a healthy human subject, it is easy to verify these facts:—

(a.) With a moderate galvanic current, muscular contraction will be produced only at the moment of completion of the circuit. During the passage of the current as well as at the breaking of it, the muscle remains at rest. [Experiment shown.]

(b.) With a stronger current, a more intense closing contraction is produced, and a weak opening contraction.

(c.) With a very strong current, a tonic contraction of the muscle is produced during the flow of the current. This experiment is too painful to be performed on the human subject, except over an anæsthetic region.

Pflüger's celebrated law of contraction is best understood by reference to the polar action of the current, without having recourse to the doctrine of electrotonus.

It thus becomes manifest how requisite it is to pay attention to the relative intensities of the currents employed in testing motility, and this leads to the next branch of the subject—*viz.*, the relations of electricity to diagnosis and prognosis, a department of which the

great practical utility is only beginning to be acknowledged by the profession at large, although it is nearly forty years since Marshall Hall laid the foundations of a novel and important method of clinical investigation. Yet to all who are conversant with the advance of medicine within the past fifteen years, it is superfluous to remark that electrical testing is almost as indispensable to the correct recognition of nutritive disturbances and paralytic conditions, as auscultation and percussion are to the diagnosis of cardiac and pulmonary affections.

Electrical examination of the motor apparatus "has already been developed into an extremely valuable means of diagnosis; it enables us to draw conclusions in regard to many palpable as well as impalpable changes in the muscles accompanying the most various motor disturbances; it not unfrequently enables us to judge correctly of the anatomical seat of the lesion; it affords us a basis for prognosis, and not unfrequently supplies indications for treatment. An accurate insight into the pathology of many motor disturbances could not, at the present time, be obtained without electrical investigation" (Erb).

Speaking of the diagnosis of nerve-injuries, the distinguished American surgeon, Weir Mitchell, says:—"My colleagues and myself had many chances of testing the value of these (electrical) means, and, finally, we learned to rely upon them with the utmost confidence."

The object of electrical investigation is to determine in a given case whether the part of the organism affected reacts in the normal or abnormal way to electricity. Since each kind of current (galvanic and faradic) can, under certain pathological conditions, produce effects which the other cannot, the conclusion is plain that no electrical examination of a case is complete, or even worth much, in which the effects of the two kinds of currents have not been tested. In like manner in electro-therapeutics the controversy as to the exclusive virtues of either the induced or galvanic current must be considered at an end, because both are indispensable in practice.

Speaking generally, the reaction of nerve and muscle to electricity may be altered either qualitatively or quantitatively.

As an example of qualitative alteration, I may adduce the fact, sometimes observed in peripheral palsies, of the positive pole producing an effect as great as or greater than the negative pole, thus inverting the normal formula. But the quantitative alterations, to

which my remarks will apply, are much the more important, and they fall under three heads:—

1. Simple increase of excitability.
2. Simple decrease of excitability.
3. The “degeneration-reaction,” to be presently explained.

Let me now try to point out what assistance we can get in these four groups of cases:—

- (a.) Central cerebral affections.
- (b.) Central spinal affections.
- (c.) Peripheral lesions of nerve trunks.
- (d.) Intra-muscular nerve lesions and myopathic lesions, between which it is difficult to draw the line pathologically.

I. CENTRAL CEREBRAL AFFECTIONS.

In cases arising from the ordinary causes (embolism, hæmorrhage, &c.) the rule is, normal electric muscular excitability, galvanic and faradic. This fact, proved by repeated observations, and easily to be confirmed anew, is of special importance in the differential diagnosis of central and peripheral paralysis of the cerebral nerves. In the early stage of some cases there is simple increase of excitability, as shown by comparison with the healthy side. Distinct and strongly marked diminution occurs only in paralysis resulting from disease of one of the peduncles of the brain, of the pons, or of the medulla oblongata. It is singular that no lesion of the cerebrum appears to have the effect of directly inducing alterations of the muscular tissue. The diminution of excitability which occurs in long standing cases of hemiplegia is, we have reason to believe, due not only to degeneration of the inactive muscles, but partly also to secondary implication of the spinal cord. In some cases of hemiplegia, as Dr. Clifford Allbutt well remarks, the degree of palsy is of little value as a test of the amount of central mischief. It is, therefore, worth while, therapeutically, to give electricity a fair trial, and a few sittings will decide the question whether any good can be effected or not.

In bulbar paralysis, or glosso-laryngeal paralysis, simple loss of excitability occurs in the *later* stages, a notable difference from what happens in peripheral paralysis.

II. CENTRAL SPINAL AFFECTIONS.

The differential diagnosis of spinal disease from peripheral lesions, presents greater difficulties than in cerebral cases. Accord-

ing to Erb, no very positive conclusions can be drawn from electrical testing, and he has met with various cases in which it was impossible to determine with certainty whether the paralysis was peripheral or spinal. But by adopting Charcot's recently published views ("Lectures on Diseases of the Nervous System," New Syd. Soc., 1877), we gain a clue to the seemingly perplexing diversities in the electrical reaction and trophic state of muscles consecutive to affections of the spinal cord. The key is to be found in the remarkable influence which lesions of the anterior nerve-cells of the cord play in the production of nutritive muscular alterations.

From this point of view spinal disorders can be separated into two very distinct groups:—

Group 1.—Those lesions of the cord which, as a rule, do not directly modify the muscular nutrition. They have one character in common—all tend to limit themselves to the white fasciculi of the cord, and if the grey matter be at times invaded, the region of the anterior cornua is respected, or at least the greater multipolar nerve-cells which occupy that region are spared.

Examples of this group are afforded by fasciculated and disseminated sclerosis, and by the partial myelitis set up by Pott's caries, tumour-pressure, &c. In all such cases the electric excitability will be normal, or nearly so, because the grey matter escapes, or is affected to a slight extent vertically. Extension of the morbid process to the motor cells of the anterior cornua would necessarily involve superadded phenomena.

Group 2, will include those lesions of the cord which almost inevitably influence, more or less rapidly, the nutrition of the muscles.

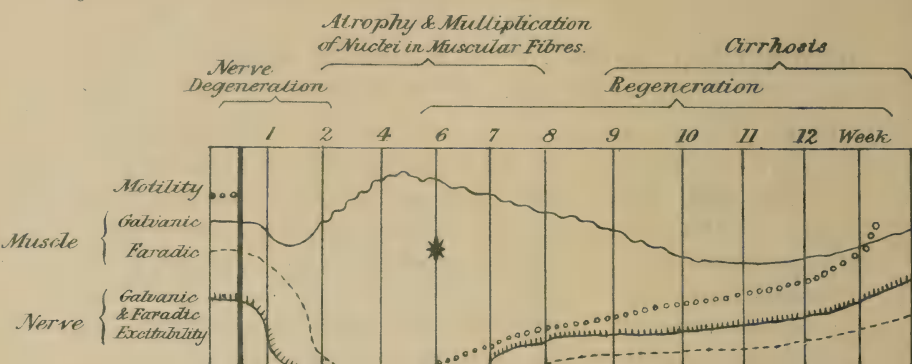
Examples of acute or subacute lesions of this nature, involving a *considerable length* of both the white and grey substances, especially the latter, are seen in central myelitis, spinal apoplexy, and diffuse irritation of the cord consequent on fractures and luxations of the vertebral column.

Infantile spinal paralysis, progressive muscular atrophy, and spinal paralysis of adults, furnish examples of the results of more delicate lesions invading, in a curiously exact way, circumscribed spots in the grey matter of the anterior cornua, with or without participation on the part of the white columns.

This group of cases is attended with marked diminution or even abolition of electric excitability.

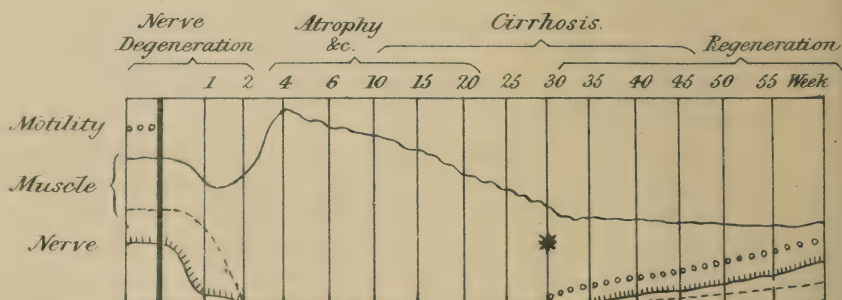
MILD CASE

(Fig. 1)



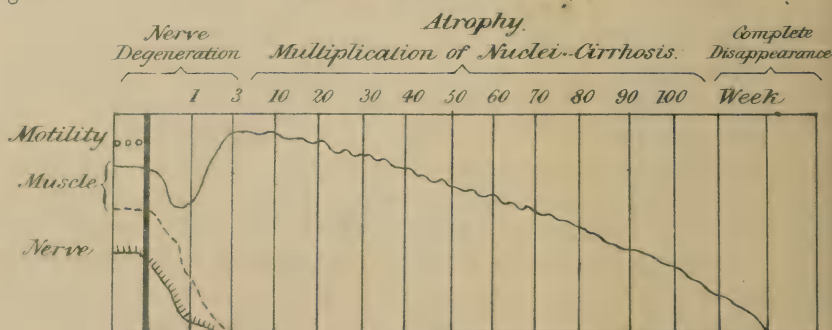
(Fig. 2.)

RECOVERY SLOW



(Fig. 3.)

NO RECOVERY



The * indicates the period of return of motility.

III. PERIPHERAL LESIONS OF THE TRUNKS OF NERVES, INCLUDING BASAL PARALYSES OF THE CRANIAL NERVES.

In this field of pathology the results of electrical investigation are of pre-eminent value, and the following summary embodies the most important of the data which we possess. Standing out prominently is a central fact, of high pathological interest, which rests upon conclusive evidence confirmed by numerous independent observers, viz.:—

Motor nerves and muscles must be separated pathologically, and they obey entirely different laws; for, while the nerve may quickly lose its irritability to all forms of electricity, the muscle may retain the capability of being excited by one form of electricity (the interrupted galvanic current).

Dividing peripheral cases into mild, severe, and intermediate groups, it may be laid down:—

(a.) In the mildest cases only—*e. g.*, in trivial cases of injury, is the excitability normal.

Rarely and transiently a slight increase of irritability may be detected.

(b.) In severe cases there is speedily complete loss of irritability in the *nerve*, followed, eventually, by total disappearance of irritability in the *muscle*, even towards interrupted galvanic currents (at least percutaneously)—a condition which admits of little or no hope of recovery (Plate II., Fig. 3).

(c.) But in a large number of curable cases a remarkable alteration in behaviour develops after the first week. This consists in certain qualitative and quantitative changes in the electrical reaction which are comprehended under the infelicitous phrase “degeneration-reaction” (Erb), and are briefly as follows:—

In the *nerve* there is rapid diminution or loss of both galvanic and faradic excitability. In the *muscle*, the faradic and galvanic excitability are likewise diminished, and the faradic excitability is even extinguished for a variable time, yet, singularly, after the second week, the galvano-muscular irritability is not only preserved, but is notably heightened for a certain time, and slowly falls to the normal level after weeks or months (Plate II., Figs. 1 and 2).

The thick vertical line in the diagrams indicates the cessation of motility, the asterisk the return of voluntary power.

These instructive phenomena are easily observed in cases of Bell's paralysis of the face, in which affection they have been

studied with especial care. Coupled with the above changes are certain qualitative alterations in the polar relations of the current, which are not of much practical consequence, and upon which it is unnecessary to dwell.

In connexion with these data three practical rules may be formulated which are of great value in relation to diagnosis and prognosis:—

(1.) Excluding the spinal paralysis of children and of adults, this degeneration-reaction is almost peculiar to peripheral palsy.

Lead palsy is, for the present, to be placed in the category of peripheral palsies, especially as even recent microscopic observations have not discovered any anomalies of importance in the spinal cord.

In doubtful cases of plumbism the electric reaction of the muscles and nerves will often enable us to determine whether lead be or be not the cause of the paralysis. On a former occasion I reported a case of generalised neuralgic and paralytic symptoms, due to unsuspected lead-poisoning, in which electrical examination led at the outset to the correct diagnosis of a case which had previously been otherwise interpreted by three other practitioners.

(2.) Whenever this degeneration reaction is well marked, the progress of the case will almost surely be tedious, and it will drag along over many weeks or months.

(3.) Electrical examination will enable us to detect and expose a simulated case of peripheral paralysis, as in a case recorded by Dr. Althaus (3rd Edit., p. 455).

IV. MYOPATHIC AND INTRA-MUSCULAR NERVE LESIONS.

To this group may probably be referred cases of muscular rheumatism, and possibly some examples of slight facial paralysis. Recent cases of muscular rheumatism exhibit normal electric excitability, but in long-standing cases, with atrophy of the muscles, their faradic excitability is impaired.

In a case of local paralysis the co-existence of normal electrical reaction of the trunk of a nerve, with altered reaction of the muscles supplied by it, would, *pro tanto*, be evidence of a primary affection having its seat in the muscular tissue, or in the terminations of the motor nerve.

From all these considerations we gather that it is impossible to form an accurate estimate of the state of the motor function in disease, or to frame a rational prognosis, without the aid of careful and repeated electrical examinations.

I proceed, in the last place, to speak of the modes in which the beneficial action of electricity is exercised, and in doing so will refer to the physiological effects only of the current, lack of time precluding me from enlarging on the physical and chemical influence of the current—*i.e.*, its thermal and electrolytic effects, which have been so happily utilised in surgery.

The physiological and therapeutical action of electricity depends essentially (1) upon the tissue or organ immediately excited—*e.g.*, brain, spinal cord, muscle, &c.; and (2) upon the mode of termination of nerves, whether sensory or motor.

At present it must be admitted that, as with drugs, so with electricity, many of the therapeutical indications have been empirically discovered, and are not grounded upon any precise knowledge of physiological or pathological processes, and the complicated effects of the galvanic current especially are by no means thoroughly understood.

The leading ascertained facts in relation to the curative powers of electricity may be provisionally grouped under these heads, *viz.* :—

1. Counter-irritation.

This is probably the primary mode of action of faradism in relieving neuralgia; especially if the current be applied by dry electrodes. Faradisation appears to be useful chiefly in peripheral neuralgia, and in theorising on its effects the influence of reflex inhibition is a contingency not to be lost sight of.

2. Modifying and Sedative Action.

Under this head we may refer to the brilliant successes obtained by the use of the constant galvanic current in neuralgia, both central and peripheral, and in some cases of muscular spasm.

In no other disease is the beneficial result of electro-therapeutical treatment so certainly established as in neuralgia; and when, after a little time, the indications shall have been rendered more precise, we cannot doubt but that a substantial triumph in therapeutics will have been gained.

Even as it is, if we compare the action of electricity with that of subcutaneous injection of narcotics in neuralgia, there is reason to conclude that, while the action of the latter is too often only palliative, and is liable to grave abuses, electricity not unfrequently plays the part of a true curative agent, and that in a speedy and sometimes astonishing manner. It has not yet been satisfactorily proved that the direction in which the current passes exercises any material influence upon its therapeutical action.

In facial neuralgia electricity has recently gained a conspicuous position; and, as Erb remarks, it may fairly be said that it is essentially owing to the extension of electrical methods of treatment that the prognosis of true *tic douloureux* is no longer so hopeless as Trousseau represented it to be.

It is worth while here to repeat the encouraging and often-quoted experience of Niemeyer. He has seen "two cases of *tic douloureux* cured by the constant current; one of them was of 30 years' standing; and in the other, eleven operations—some of them severe ones, such as the ligation of the carotid, resection of the superior maxilla, &c.—had been performed without benefit."

In lumbago and sciatica again, the commonest forms of neuralgia, the results of galvanisation are, in most instances, extremely favourable, and even in obstinate cases my own experience confirms that of other observers that it rarely fails to give relief. The intractable nature of some of the forms of neuralgia in which electricity has been found most distinctly useful is acknowledged; and since their usual course and relations to ordinary means, such as medicinal sedatives and narcotics, are tolerably well known, clinical observation is the better entitled to speak with confidence of the success of the electrical treatment.

The most striking example of the powers of electricity in relieving spasmodic affections is afforded in writers' cramp, and some remarkable cases of success are reported from reliable sources. Electricity has, at least, nothing to fear from contrast with the utter inefficacy of all medicinal treatment in this curious disorder.

In respect to locomotor ataxy, it is barely the truth to say that electricity has accomplished something to lighten the gloomy prospects of the sufferers from this disease, and that it has blunted the sting of Romberg's dread sentence upon these unhappy patients.

But just in proportion as the sedative action of the current is duly recognised, it becomes increasingly necessary that caution should be observed not to push its calmative influence too far; and the danger of inducing exhaustion in cases of spinal irritation, locomotor ataxy, &c., is not only a theoretical possibility but an ascertained fact.

3. Direct and Reflex Stimulation.

This, the exciting action of electricity, is that most commonly utilised, and is illustrated in the ordinary applications of faradisation.

(a.) *Sensory nerves.*—In anæsthesia electricity is indisputably the

most effective remedy, and under the repeated use of faradisation of the dry skin with the electric brush, Weir Mitchell testifies that "it is common to find sensation returning to regions which are deficient in every form of sensory life." In certain cases of cerebral hemiplegia, attended with nearly complete anæsthesia, Vulpian has obtained surprising results from cutaneous faradisation, but in loss of sensation, due to alterations of the peripheral nerves—*e. g.*, in long-standing cases of saturnine anæsthesia, it would be vain to expect benefit from electricity (*Arch. de Phys.*, 1875).

(b.) *Motor Nerves and Muscles*.—In many cases of paralysis electricity appears to forcibly overcome, sometimes suddenly, some unknown obstacle to the transmission of stimuli to the muscles, and occasionally motility is restored so rapidly—it may be after one application—that it seems as if some functional torpidity alone barred the way. Experiments on man have further demonstrated that electricity increases the excitability of nerves, and the *refreshing* or *restorative* effects (Heidenhain) of the galvanic current have probably, also, a beneficial influence upon muscles in a state of fatigue. When to these influences we add those of compulsory muscular exercise, and the increased afflux of blood to the parts operated on, we are enabled to glean some idea of the modes in which the anti-paralytic action of electricity is exerted.

4. Lastly, Trophic Influences.

Electricity unquestionably promotes the processes of regeneration in nerve and muscle; and in wounds of nerves we have Weir Mitchell's high authority for recommending the use of the faradic or galvanic current from the earliest date at which the healing of the wound allows of their use. His experience has convinced him that the common practice of leaving the muscles alone for months until the nerve has had time to undergo a process of repair and reunion is incorrect (*Injuries of Nerves and their Consequences*, 1872).

Electricity, moreover, accelerates the osmotic movements and chemical changes in living tissues, and the salutary effects of the galvanic current upon the stagnant circulation of paralysed limbs is sometimes truly astonishing. Dr. Russell Reynolds, for example, refers to two cases:—"In one there had been, for many months, dusky blueness of the fingers and hands, the result of cardiac disease and vascular change, but this colour was removed by one application, and has not since returned. In the other, a similar discoloration had persisted, after an attack of erysipelas, in a paralysed limb, but it was removed as quickly and persistently."

Although patience and perseverance in practice are indispensable requisites in the electric treatment of paralysis, it is equally necessary to bear in mind the danger of doing serious harm by over-stimulation, and it is to be regretted that occasion is still given to reiterate that caution should be used in employing "an agent which appears capable of inducing, increasing, reducing, or destroying the functions of both muscles and nerves."

In conclusion, it appears impossible for any one who is practically conversant with the known facts and laws of electro-therapeutics to avoid the profound conviction that in electricity medicine possesses a trusty and powerful ally, without whose timely aid some of the nicest problems in diagnosis and pathology must remain unsolved.

In the therapeutics of nervous diseases we even now have at our command a remedy of surpassing efficacy, for which, beyond doubt, in the immediate future opens out a widening field of the highest promise, and one which will not "partake of the fate of ephemeral remedies, which love of novelty introduced and caprice abandoned."

ART. IX.—*On the Relation between the Distribution of Cholera in Dublin during the Epidemic of 1866 and the Geological Structure of the Dublin District.*^a By THOMAS WRIGLEY GRIMSHAW, M.A., M.D.; Physician to Steevens' Hospital, and Consulting Physician to Cork-street Fever Hospital.

IN May, 1875, when reading Dr. Hime's translation of Professor Pettenkofer's observations on Cholera, it occurred to me that a careful investigation of the relations between the distribution of cholera in Dublin and the geological structure of the site upon which Dublin stands, might tend to throw some light on the promoting causes of cholera, and serve also as a test of the truth of some of the views put forward by Professor Pettenkofer. In writing a review of Dr. Hime's useful little work (from which my quotations of Pettenkofer's views are derived) for *The Dublin Journal of Medical Science*, I appended a note concerning the geological structure of the Dublin district.^b At the time I wrote that note I had not before me any of the details of the distribution of cholera in Dublin and its suburbs, except those published in 1867, in the second edition of Dr. Mapother's *Lectures on Public Health*.

^a Read before the Medical Society of the College of Physicians on Wednesday, March 6, 1878. [For the discussion on this paper see page 357.]

^b Dublin Medical Journal. Vol. LIX., pp. 437 et seq.

In the following remarks I have endeavoured to add something to the valuable researches of Dr. Mapother upon the distribution of cholera in Dublin, and at the same time to test the truth of Pettenkofer's views with regard to the relation which dampness of soil bears to the prevalence of cholera. Although these results may not be quite as precise as they might have been had the investigation been carried out during the epidemic of 1866, or immediately after its termination, or had Dr. Mapother at that time had in his possession the information which has since been collected by the officers of the Geological Survey, yet I think some little interest will attach to the observations which I am about to make.

I have to consider, first, the relation of Pettenkofer's observations to the inquiry I have undertaken. Pettenkofer divides the promoting causes of cholera as follows:—

1st. Conditions of traffic.

2nd. Conditions of place (geological formation, &c.).

3rd. Conditions of time (season).

4th. Conditions of individual.

In his works he considers the means by which the cholera germs are carried from place to place, the season of the year in which it spreads with greatest facility, and the conditions which make particular individuals prone to the disease. With these matters it is not my present purpose to deal, but with the *second condition*—namely, that of place. Pettenkofer, referring to the relation between the presence of “ground water” and the production of enteric fever, says:—“Just as in certain places the existence of typhoid fever exhibit a certain temporal dependence on the variations in the moisture of the soil of the so-called ‘ground water,’ in like manner it is probable that cholera is similarly dependent, though, owing to the fortunately less frequent prevalence of cholera, the relationship cannot be so regularly and satisfactorily proved as in the case of typhoid fever.”

Regular observations on the variations of ground water are and have been kept for many years at Munich under Pettenkofer's directions, and the results of his comparison between the variation in ground water and enteric fever are so well known that I need not refer to them here. Pettenkofer shows that “the variations in the level of the ground water in the porous stratum are important only as showing the variations in the moisture of the superincumbent layers. The importance of these variations in moisture consists in their facilitating or retarding certain organic processes

in the soil, while the ground water itself may be quite harmless and innocent in the matter. The ground water is at present the most accurate measure we have for the variation in the moisture of the soil.

“Munich, with a population of about 170,000, is situated on the Isar; the body of water is not great, and the river has considerable velocity. As a rule very few families occupy an entire house, it being customary to occupy a flat, as in many other Continental cities. Water-closets are a rarity there, and the privies are connected with cesspools of considerable size to meet the requirements of the number of persons in each house. The surface is level, being part of a large plain extending many miles on both sides of the river. The upper stratum is very permeable to both air and water. The city is built, for a great part, on gravel composed of mountain (Alpine) limestone. This stratum, varying in thickness from twenty to forty feet, rests on a stratum of marl, the depth of which has not yet been ascertained, but it must be at least several hundred feet. Within the city the marl, for the most part, does not reach the surface except on the steep, right bank of the Isar. It is the deposit of an antideluvian inland sea which covered the present elevated plateau of Bavaria during the tertiary period. It is impermeable to water, and forms the watertight substratum for the whole district. It constitutes also the floor of the river bed, the ground water collecting on it and gravitating on both sides towards the river. The river bed is the deepest point of the watertight substratum of marl throughout the district, and therefore the ground water drains from both sides towards it.” Thus the houses in Munich are built on a porous gravel bed permeated by air and water.

Pettenkofer points out the advantages and disadvantages of this state of things—the former being that it confers immunity from damp walls, that the rain falling on the surface of the ground quickly permeates and passes away, and the fall of the ground water towards the river tends to keep the wells fresh, as it were, by a running underground stream. The disadvantages are, that the spaces between the gravel afford places for the accumulation of foul air and foul water derived from excrementitious or decomposing matters cast upon the surface.

Having given “Munich” as an example, and quoted from the Bavarian Cholera Commission of 1854, Pettenkofer shows that Munich has a district on the right bank of the Isar insusceptible to

cholera, being situated on clay. In Munich, Haidhausen, and Berg, the epidemic of cholera confined itself to the houses situated on the gravel, and avoided in a most striking manner those situated on the more elevated clay.

So far for the condition of Munich, situated on a permeable gravel bed and with an adjoining district standing on impervious clay. To compare Munich with Dublin, we find that in Dublin and its suburbs these conditions are fulfilled, although in a somewhat different manner, and I shall presently show with what result.

I have limited my investigations to Dublin and its immediate suburbs—namely, the city; Clontarf, Drumcondra, Finglas and Glasnevin; Kilmainham and Chapelizod; Rathmines and Rathgar; Donnybrook, Sandymount, and Ringsend (Pembroke District). Some short time since it was generally considered that the *whole* of this district consisted of boulder clay resting on limestone; and when I first considered this question it appeared to me that, as this boulder clay is an impervious stratum, the experience of Dublin was at direct variance with the experience of Munich. On further inquiry, however, by the kindness of Mr. Hull and Mr. Cruise, of the Geological Survey, I find that recent observations had shown that this was not true, but that a considerable portion of the districts is situated on gravel which is superimposed on the stratum of clay previously mentioned. This gravel bed was once the bottom of the sea, and consists of a raised sea beach which extends along a considerable portion of the east coast of Ireland. This “littoral” gravel bed, commencing at the extreme north of the Dublin district, as I have defined it, at Dollymount, forms a narrow strip running along the Clontarf Strand; it then passes more inland along the estuary of the Tolka up to Drumcondra, including the suburbs of Richmond, Fairview, and Ballybough. On entering the city its boundary line passes slightly south of Summer-hill and Great Britain-street, in some cases crossing the latter street, crosses the upper end of Capel-street, and gradually slopes southwards to the lower end of Smithfield, passes just in front of the Royal Barracks along Parkgate-street, inside the Park wall along the Conyngham-road; the boundary crosses the Liffey at Island-bridge, turns towards the south-east, past the north-east corner of Steevens’ Hospital, along the face of the slope between James’s-street and the Liffey, reaching to St. Audoen’s Church, at the back of Christ Church, crosses the top of Parliament-street, passes along the back of the houses on the north side of Dame-street, crosses College-

green at the Chamber of Commerce, then follows the lines of Suffolk-street, Nassau-street, and Clare-street, passes through the gardens of the houses on the north side of Merrion-square and Lower Mount-street. On crossing the Grand Canal at Lower Mount-street bridge, the line takes a sudden turn to the south, crosses Upper Baggot-street to the east of Waterloo-road, runs at the back of the houses in Waterloo-road, then crosses to the south-east through Clonskeagh, and ends at Williamstown. Besides these, there are other small alluvial gravel beds on the Tolka above Drumcondra, on the Liffey above Island-bridge, and on the Dodder above Milltown.

It is thus evident that a considerable portion of the city and suburbs included in the above boundary line is situated on a porous gravel bed, whereas all outside the boundary line is situated on the impervious boulder clay, excepting the alluvial gravel beds above mentioned.

Taking the different registration districts of the city and suburbs, we find that—

I. In Clontarf there is only a small portion of the district on gravel, but nevertheless a considerable number of the inhabitants reside on this portion.

II. In Drumcondra a small portion only, but it also is populous.

III. In Glasnevin District there is a small portion of gravel composed of the Tolka alluvial deposit.

IV. No. 1 North City District—0·63 of the inhabited area is situated on the gravel bed.

V. No. 2 North City District—0·33 of the inhabited area is situated on the gravel bed.

VI. No. 3 North City—0·17 of the inhabited area is situated on gravel.

VII. No. 1 South City—Only a small portion along the river is situated on gravel.

VIII. No. 2 South City—0·45 of the inhabited area is on gravel.

IX. No. 3 South City—None of this district stands on gravel.

X. No. 4 South City—0·59 of the inhabited area is on gravel.

XI. Rathmines—Not any portion is on gravel.

XII. Donnybrook—Nearly all is on gravel.

XIII. Palmerstown has only a small portion on Liffey alluvial gravel bed.

We have thus mapped the city and suburbs according to the pervious or non-pervious nature of its soil, and we find that every

variety exists as to the distribution of the population over the gravel bed—some districts do not include any, others a great extent of pervious strata. To make an inquiry of this sort completely accurate, it would be necessary to ascertain the exact number of persons who resided upon and not upon the pervious strata, and the relative prevalence of cholera among each class. This information could only have been correctly obtained at the time of the epidemic, and I therefore have to proceed as best I can without it.

By the kindness of the Registrar-General, I have obtained an accurate list of the deaths, dates of death, and residences at the time of death, of all the persons who died of cholera in Dublin and its suburbs during the epidemic of 1866. The general result is shown in the following Table:—

Showing the Number of Deaths and the death-rate per 10,000 living, from Cholera, the proportion of the inhabited area situated on gravel, and the estimated population in 1866 of each of the Registration Districts in the City of Dublin, and its suburbs.

1	2	3	4	5	6	7	8	9	10	11
Name of District	July	August	September	October	November	December	Total	Deaths per 10,000	Proportion of Inhabited Area on Gravel	Estimated Population, 1866
I. Clontarf, - -	-	-	6	-	-	-	6	21·37	Very small	2,807
II. Drumcondra, -	-	-	1	2	-	-	3	7·48	Very small	4,012
III. Finglas and Glasnevin	-	-	1	6	1	-	8	12·81	Do. on Tolka	6,246
IV. North City No. 1,	-	12	31	67	32	3	145	30·11	0·63	47,755
V. „ 2,	-	8	40	30	18	1	97	28·86	0·33	33,818
VI. „ 3,	-	5	32	23	13	1	74	27·79	0·17	27,022
VII. South City No. 1,	-	14	24	26	41	7	112	35·55	Very small	31,505
VIII. „ 2,	-	27	38	33	26	-	124	41·13	0·45	30,100
IX. „ 3,	-	5	24	37	40	4	110	29·24	0·00	37,957
X. „ 4,	3	14	37	75	49	6	184	41·51	0·59	41,928
XI. Rathmines, -	-	-	5	11	20	3	39	21·01	0·00	18,560
XII. Donnybrook, -	-	6	33	69	12	5	125	61·25	All	20,482
XIII. Palmerstown, -	-	-	-	3	27	5	35	51·47	Small on Liffey	6,800

A study of the above Table shows some remarkable results. On

comparing columns 9 and 10, it is at once apparent that the death-rate from cholera was greatest in the Donnybrook district, which all stands upon gravel; next greatest in No. 4, south city (except in the case of Palmerstown, where the smallness of the population exaggerates the rate), nearly the half of which stands upon gravel. That in Rathmines, a densely populated district standing on impervious soil, the death-rate was very low. The only districts which escaped cholera more effectually than Rathmines being Finglas, and Glasnevin and Drumcondra, in both of which there is but little pervious strata, and such very small populations that a slight epidemic outbreak causes a misleading death-rate. On the north side of the city it will be seen that the highest death-rate occurred in No. 1 district, containing most pervious strata; No. 2 stood next, with the next largest tract of pervious soil; and No. 3 last, with the least amount of pervious strata. It will be seen, however, that the relation between the pervious strata and the death-rate from cholera is not proportional, and that there is something very exceptional about district No. 3, north. On the south side of the city we find similar exceptions to those on the north. Thus, district No. 3, south, none of which stands on pervious strata, has a very high death-rate, though not as high as the others on the pervious strata, and in No. 1, south, a high rate, with a very small amount of pervious strata. In Palmerstown again we find a high death-rate, with but little pervious strata. While Pettenkofer's view is supported by the distribution of cholera in most of the districts, it is quite apparent that the condition of things in No. 3, north city, Nos. 1 and 3, south city, and Palmerstown, seem inconsistent with these views. These, however, all admit of explanation in a way which, I think, is not inconsistent with Pettenkofer's views, and which is almost altogether afforded by Dr. Mapother's investigations into the relation between the old river courses of Dublin and the distribution of cholera in the *city*.

The state of things in No. 2, north city district, is sufficiently explained by the course of the Bradogue (or Bathogue) river flowing through the portions of the district in which cholera chiefly prevailed. The prevalence of cholera in Nos. 1 and 3, south city, is also explained by Dr. Mapother's observations. The Poddle river winds its tortuous course through districts Nos. 1, 2, and 3 of the south city. To it may be attributed the prevalence of cholera in No. 3, although the extremely unhealthy conditions of this district are sufficient to account for the prevalence of any epidemic disease,

and there can be little doubt that No. 2, with its small share of pervious strata, has the difference made up by the presence of the Poddle and other old water-courses described by Dr. Mapother. In district No. 1, south, cholera prevailed either on the small portion of the great littoral gravel bed, which is situated within the district, or in the hollows bordering the Camac river in Bow-lane, or on the Poddle, as in district No. 3, and part of district No. 2. From the foregoing sketch it is evident that the greatest number of deaths from cholera occurred either on pervious strata or close to old river courses. Dr. Mapother indeed seemed to be of the opinion that the distribution of the disease could be explained almost altogether by the presence of the old river courses. A careful consideration of the conditions produced by one of the old river courses shows that they are very similar to those produced by the presence of a pervious stratum such as the gravel beds of Dublin. The old river course contaminated by sewage with houses built *in* it, over it, and alongside it, with porous foundations, and possibly standing on the *débris* of old houses, bears much the same relation to those houses that a gravel bed does to the houses built thereon. So far my statements have been of a general nature relating to large districts, but to prove the cases put forward by Pettenkofer and Dr. Mapother, it is necessary to show that cholera *spread* over the gravel strata of Dublin, and along the old river courses, and *nowhere else*, or if anywhere else, the particular circumstances must be explained.

It is necessary to admit, in the first instance, that cholera is a contagious disease, and that it will spread if suitable conditions are provided, as laid down after Pettenkofer at the beginning of this paper. It may be assumed that every portion of Dublin and its suburbs had, at some time or other during the cholera epidemic, cholera poison introduced into it. The introduction of the disease cannot, of course, be proved in all instances, but where a case of cholera arises, and the patient dies, the evidence of the introduction is conclusive. If the disease stops with the first case, or only attacks one or two others in the same house, and does not extend in the vicinity, it may be assumed that the local conditions of that neighbourhood are unfavourable to the spread of the disease, and *vice versâ*. This has been specially noticed by Dr. Mapother, who remarks that the occurrence of an *isolated* case of cholera in a neighbourhood is the best proof of the resistance that that neighbourhood offers to the spread of the disease.

I shall now consider each of the thirteen Dublin districts separately, with a view of seeing to what extent the disease spread in each:—

I. Clontarf.—In this district there were but *six* deaths from cholera; two of these were isolated cases, one at Dollymount, the other at Clontarf. Four cases in a group occurred at Killester, the deaths taking place on the 6th, 11th, 12th, and 14th of August. This outbreak seems to be connected with Killester quarry. The estimated population, in 1866, of the district was only 2,807, so that the six deaths cause a misleadingly high rate.

II. Drumcondra.—There were but three deaths in this district, and all were isolated cases. The disease did not spread anywhere.

III. Finglas and Glasnevin.—Eight deaths occurred in this district; all were isolated except five, which occurred on the 4th, 10th, 15th, 27th of October, and 2nd of November, at Finglas, on the gravel along a tributary of the Tolka, and at Glasnevin on the alluvial gravel of the Tolka. Here the disease only spread on the pervious stratum, and along the river bed at Finglas.

IV. North City, No. 1.—There were 145 deaths, or at the rate of 30·11 per 10,000 of the estimated population. Of these deaths 118 occurred on the pervious, and *not more* than 27 on the impervious stratum; owing to a confusion of names, it is probable that many of these 27 occurred on the pervious strata, as 11 deaths were reported as occurring in White's-lane, there being three White's-lanes, two situated on, and one not on gravel. Not only did so large a number of the deaths occur on pervious strata, but in only one instance did the disease spread on the impervious strata—namely, in Lower Dorset-street, where four deaths occurred in one group. The cases in the White's-lanes appear to have been arranged in groups, but as these are uncertain they must be left out of consideration. All the deaths, with few exceptions, which occurred on the pervious strata, were arranged in groups—1st, about the North-strand; 2nd, about Mecklenburgh-street; 3rd, about Mayor-street; and a very small group, possibly including one of the White's-lanes, in Marlborough-street. The conclusion derived from the experience of this district is that, with one exception—namely, that of Dorset-street—cholera only spread among persons living on the littoral gravel bed. The Dorset-street outbreak is explained by the presence of an old river course mentioned by Dr. Mapother, and marked on Rocque's map of Dublin (1756).

Dr. Mapother refers to pools marked on Rocque's map at Gregg's-lane (Marlborough-street), Market-street, &c., where cholera prevailed. These pools have been filled up, but, so far as the public health is concerned, they are only portions of the gigantic underground pool which is now known to occupy the centre of Dublin, and which at these points was visible, owing to hollows in the surface. The filling up of such pools can have done nothing to improve the health of the city, as it was only hiding, not destroying them, and forming additional sites for dangerous houses.

V. North City, No. 2.—In this district there were 97 deaths from cholera, or at the rate of 28·86 per 10,000 of the inhabitants. About one-third of the inhabited area of the district is situated on pervious strata, and but a small portion is traversed by the Bradogue river and its tributaries. Of the deaths, 55 occurred in persons residing on the pervious stratum, and 42 in those residing on the impervious soil. Of the 42 deaths, 5 were isolated cases, more occurred in Dorset-street and Wellington-street, and were part of the outbreak mentioned in No. 1 district, as arising in connexion with the tributary of the Bradogue. Another outbreak on impervious strata occurred in North King-street and the adjoining streets and lanes; this outbreak caused 23 deaths. Another small outbreak occurred in Bolton-street, causing 3 deaths. These two latter groups are also, according to Dr. Mapother, on the course of the Bradogue and its tributary. The outbreak in Bolton-street was insignificant. The most formidable outbreak on the pervious strata was that which occurred between Abbey-street and the quays, in this district at least 24 deaths occurred. Another serious outbreak occurred in Greek-street, Bull-lane, and the neighbourhood, also situated on the pervious strata. Britain-street bounds the littoral gravel bed on the north, and along the portions of this street, situated on the gravel, an outbreak of cholera occurred.

VI. North City, No. 3.—In this district there were 74 deaths, or at the rate of 27·79 per 10,000 inhabitants. Only 0·17 of the inhabited area is situated on gravel. Of the deaths, 44 were of persons residing on pervious, and 30 of persons residing on impervious strata. Of the 30, 5 were isolated cases; 5 were either on or close to the boundary of the gravel. A serious outbreak in "old" or "north" Church-street, Brunswick-street, and Constitution-hill, caused at least 14—probably 16 of the deaths. This outbreak seems to be connected with the presence of an old river course, said to be a tributary of the Bradogue, and it is certainly

clear of all pervious strata. 5 other cases form part of the North King-street outbreak already referred to.

On the pervious stratum at the lower end of Bow-street *nine* deaths occurred; also groups of cases arise at the lower end of Beresford-street, in Greek-street (causing 10 deaths); Mary's-lane (causing 11 deaths); and part of Queen-street—only 3 isolated cases occurred, and 2 of these were probably connected with neighbouring groups. Thus, except in the case of Old Church-street, the disease spread only on pervious strata.

VII. South City, No. 1.—In this district 112 deaths occurred, or at the rate of 35·55 per 10,000 of the estimated population. Only a very small portion of the inhabited area of this district is situated on pervious strata; therefore, as I pointed out at the commencement of this paper, its case is exceptional. 16 of the deaths occurred on the very small portion of gravel bed along the Liffey. One outbreak at Mullinahack and John-street caused at least 12 deaths. The outbreaks which occurred off the gravel were—1st, on the Poddle, at the upper end of the Coombe; this group comprised not less than 28 deaths. The water from the Poddle was, at this point, constantly used for domestic purposes, it being then uncovered in many places; besides this, many of the houses stood over the river, and had their walls built in this filthy stream, which is nothing more than a large sewer. The Poddle often overflowed and flooded houses in its vicinity, so that ground water was abundant here, and the little stream had even produced a small alluvial gravel bed of its own. 2nd. In the South Dublin Union Workhouse, where no less than 14 deaths occurred (some, no doubt, introduced cases). 3rd. On the Camac, at Bow Bridge, 13 deaths occurred, all connected with the bed of the Camac, a filthy and low-lying stream, also used in 1866 for domestic purposes. Thus, in this district the disease only spread on the gravel or close along the filthy streams; all other cases were isolated, except in the instance of the South Dublin Union.

VIII. South City, No. 2.—In this district there were 124 deaths, of which 39 occurred upon pervious, and 85 on impervious strata. The proportion of inhabited area in this district, standing on pervious strata, is 0·45, or a little less than one-half. In this district we again have the Poddle passing along Patrick-street; an old mill stream passes through Back-lane and Plunket-street, Bride's-alley, and Ship-street, to the site of the Old Castle ditch, and joins the Poddle in Patrick-street. In the Plunket-street

neighbourhood 18 deaths occurred, the disease having spread quickly and fatally, and holding out through the whole epidemic. Along the Poddle in Patrick-street and Ship-street at least 22 deaths occurred. These outbreaks are connected with the ground water of the Poddle and its little tributary. In this district we have one of the most striking examples of how the presence of gravel bed favours the spread of the disease. A man, several members of whose family died of cholera, on City-quay, went to Cook-street, situated in this district, to buy a coffin; when in Cook-street he showed marked symptoms of cholera, of which he died. Immediately after his visit, cholera spread with great virulence in Cook-street, causing at least 20 deaths in the immediate vicinity, and, no doubt, being the origin of the Mullinahack and John-street outbreaks. Also many isolated cases occurred on the impervious strata in the vicinity, and, no doubt, were imported from Cook-street and Mullinahack. It is probable that this street, being a place where a large number of the coffins for the poor are purchased, became one of the most fertile *foci* of the disease.

IX. South City, No. 3.—This district presents the quite exceptional feature that there is not to be found within its area a gravel bed of any kind. The deaths, however, were 110, or at the rate of 29·24 per 1,000. On looking over the list of deaths it will, however, be found that 43 were isolated cases. The outbreaks were confined to five places; one of these, comprising 11 deaths, is a portion of the group connected with the Poddle where it crosses Bride-street; another group arose in Charlemont-street, and caused 11 deaths; 14 deaths occurred in Aungier-street, Bishop-street, and Upper Mercer-street. It is generally believed that an old river course, which was constructed to supply Trinity College with water, passes under the sites of those outbreaks. I believe there is scarcely any doubt about the presence of this old river at these points. In the end wall of a house in Bishop-street can be seen an arch which, I believe, spanned this stream. The only other group in this district occurred on the Poddle from Black Pitts along New-row and New-street.

Before leaving the Poddle it is worthy of remark that it passes through portions of three districts, and that its course was marked during the cholera epidemic of 1866 by a constant and virulent prevalence of the disease along its course; and that while the poor people who resided on or beside it suffered terribly from the disease, other equally poor people living under circumstances quite

as unhealthy escaped the disease, or, when it appeared, it did not spread. The Coombe is a remarkable illustration of this. This street is a filthy and crowded one. It is of considerable length, is situated in a hollow, and is a constant hot-bed of zymotic disease; yet in this street, where all the conditions favourable to the spread of contagious zymotics exist, cholera only spread at the end where it touches the Poddle. Meath-street, a most unhealthy locality, almost escaped cholera; so also Francis-street and several others which I could mention, and which I have mapped as favourable to the spread of fever and small-pox.

X. South City, No. 4.—In this district there were 184 deaths, or at the rate of 41·31 per 10,000 of the population. Over one-half (0·59) of the inhabited area of this district is situated on the littoral gravel bed, and this is inhabited by the poorest and most closely-packed portion of the population. A portion of this district—that between the line of Nassau-street, Merrion-square North, Mount-street, and the river—is almost completely saturated with ground water. The mortality in this area was far beyond the 41·31 which represents the general rate for the whole district. Of the deaths, 135 occurred on the gravel, in a population which I estimate at 21,000, or at the rate of 64·29 per 10,000. The 49 deaths on the impervious strata occurred in an estimated population of 21,000, or at the rate of 23·33 per 10,000 of the inhabitants. Of these, 31 deaths occurred in three groups; 6 in Clarendon-street and Clarendon-row, and 10 in Duke-street and at the corner of Duke-street and Grafton-street. The old river course passing to Trinity College, which produced the outbreak in Bishop-street, is believed to pass by the upper end of Clarendon-street, and follow the backs of the houses at the eastern side of Grafton-street. This *might* account for the Duke-street outbreak. An old pump in Duke-street has been, I believe, truly assigned as the cause, and the water of this pump, finding its way into the milk of a neighbouring dairy, I believed to be a promoting cause of the disease. Probably the pump is near Provost Baldwin's watercourse, and, it may be, hence the name of the College Dairy; but at all events the Duke-street outbreak of cholera occurred very near, if not on, the College watercourse. Another outbreak causing 10 deaths occurred at Power's-court and adjoining lanes between Upper and Lower Mount-street. This outbreak was on the very verge of the gravel bed, and probably on an outlying corner of it. A few cases occurred in a group in the lanes at the re-re of Leeson-street and

Pembroke-street. These are generally attributed to the use of foul water from a pump in this neighbourhood, and it is said an old watercourse passes through this district.

XI. Rathmines.—This district contains neither old river courses nor pervious strata, except a small portion of the Dodder gravel and tributaries of the Swan river; accordingly we find that there were but 39 deaths from cholera in this district, or at the rate of 21·01 per 10,000 of the estimated population; of these all but nine were isolated cases. These nine occurred in four groups—three groups of two, and one group of three—the latter in Church-lane, in the vicinity of a tributary of the Swan brook, and one of the groups of two at Classon's bridge, on the Dodder gravel bed. Thus the evidence afforded by Rathmines, as contrasted with the other districts of Dublin, is strongly in favour of Pettenkofer's views.

XII. Donnybrook.—This may be considered as identical with the Pembroke Township; nearly the whole district is situated on the gravel bed. The deaths numbered 125, or at the rate of 61·25 per 10,000 of the estimated population; of these but four cases, all isolated, occurred on impervious strata; all the others were on the gravel—the most destructive outbreaks being at Ringsend and Donnybrook.

XIII. Palmerstown.—This district contains but a small portion of alluvial gravel at Palmerstown; the deaths in this district numbered 39, or at the rate of 51·47 per 10,000 of the estimated population—the population, however, is so small (only 6,800) that this rate is much exaggerated. Of the deaths, 11 occurred in one group on the Liffey alluvial gravel at Palmerstown, and two on the littoral gravel at Island-bridge; three more occurred on the Camac river bed at Golden-bridge; seven at the second lock on the Grand Canal, and five at Ballyferment, and five more on the Crumlin road.

I think the foregoing remarks confirm, to a great extent, the observations of Pettenkofer, and point to the dangerous ground upon which a large portion of the city of Dublin and of its suburbs stands. This dangerous ground is steadily becoming more dangerous as the saturation of the gravel with sewage increases. It is therefore essential to the health of the people of Dublin that sewage should be kept out of the gravel bed, and further that all communication between the poisonous atmosphere contained in the interstices of the gravel and that of the houses built thereon should be

cut off. It is not my duty to point out the remedies, but I may venture to remark that any system of main drainage for Dublin which does not effectually prevent the admission of sewage to the gravel bed, will fail materially to improve the health of the inhabitants of the Dublin District.

ART. X.—*Obstetrical Report of the Cork Maternity for the Five Years ending December 2nd, 1877.* By H. MACNAUGHTON JONES, M.D., &c.; Physician Cork Maternity; Surgeon County and City of Cork Hospital for Diseases of Women and Children.

THIS institution has been open for the past five years and four months. It may be of interest to briefly classify the cases which have been attended since its opening. I do not wish to enter into the vexed question as to the relative advantages and security to life afforded by large lying-in hospitals and extern maternities. Statistics are every day settling this bitterly contested point. I think it desirable to give the following analysis of cases attended in the Cork Maternity, as I have taken them from our obstetric register. I must apologise if the particulars are incomplete. At least they are accurate, and serve to show the class of cases which has been assisted by our staff and nurses. I may, perhaps, with advantage, say a word on the origin of the Maternity and the field in which it carries out its operations. Previous to December, 1872, there was but one other institution of the kind in Cork, with its city and suburban population of about 80,000. This opened its door also to the county, and is named the City and County of Cork Lying-in Hospital. Many years since there was a second lying-in hospital for the south side of the city, but it has long ceased to exist. The poorer classes in the city were mainly attended to by ignorant and frequently drunken old midwives, who called in no physician until they had exhausted every traditional and superstitious device to bring on labour and effect delivery. Thus, the unfortunate woman was often kept for days in labour before the dispensary physician was called to render assistance. Only in extremity did these old "Joans" and "Tibs" suffer their dignity and "experience" to become compromised or their "practice" criticised by the assistance of a doctor. The Lying-in Hospital, imperfectly maintained as it has always been, did its work well, as far as its resources would enable it. It is only necessary to allude to the senior member of the staff, Dr. J. R. Harvey, Professor of Midwifery in the Queen's

College, Cork, to prove the class of obstetrical work done in this time-honoured hospital. Wisely, I think, it has never been attempted to extend its sphere of usefulness by enlarging its number of beds or otherwise, for its statistics amply prove the benefit of the small or cottage lying-in hospital, which it virtually is. The number of cases admitted in the year averages, I believe, between 350 and 400. The design of the Maternity was twofold. To give efficient aid early in labour, and at any moment, to the poorer women throughout the city, and to train a better class of nurse. Up to the present, with many difficulties, and very slender resources, the Maternity has fulfilled both these objects. During the first five years 1,611 cases have been attended to in this city, and some twenty highly intelligent and competent nurses have been trained. The students of the Queen's College have largely availed of the Maternity to learn practical midwifery, and there is yearly a course of lectures on obstetrics and gynæcology delivered to the class. It may be gathered from the foregoing remarks that most of the cases applying for relief at the Maternity would be from the very poorest. Often they had been for days in the hands of hostile nurses, who saw in the Maternity nurse a natural enemy, who was being taught, as she fancied, to deprive her of her means of livelihood. Only at the last moment, and then often against her wish, would she consent to send for assistance; for it took some time to educate the people to send, in the first instance, to the Maternity for help, and to have the names entered on the books. Thus, many of our cases were placed under the most unfavourable circumstances for recovery—poverty, filthy surroundings, overcrowded dwellings, delay, rude and barbarous interference, drunken friends, and little help in times of emergency, were the principal difficulties we had to contend with. Still it is strange that, during those five years, we have not lost a single case from puerperal fever coming on idiopathically, or having a purely septicæmic origin. I regret that the obstetric register does not enable me to give more complete information than is contained in the following statistics; at least the other particulars entered are not sufficiently important to add any interest to them.

STATISTICS.

Total number of Labours:—Natural, 1,267; difficult, 108 (including 6 face presentations); præternatural, 74; complex, 117; abortions and miscarriages, 45; total, 1,611.

Praternatural.—Upper extremity, 14; breech and lower, 60; total, 74.

Complex Cases.—Twins, 34; triplets, 1; placenta prævia, 5; post-partum hæmorrhage and retained placenta, 35; accidental hæmorrhage, 5; funis, prolapse of, 16; convulsions, 5; rupture of uterus, 3; adherent placenta, 12; hydatids, 1; total 117.

Operations.—Forceps, 93; versions, 17; craniotomy, 3; evisceration, 2; hysterotomy, 2; total, 117.

Still-born children at full time from all causes, 64; abortions and miscarriages, 45; deaths of children after instrumental delivery, 12.

Deaths of mothers, 14.

Causes of Maternal Deaths.—Consumption, 2; concealed hæmorrhage (accidental—one case dead on arrival of physician) 2; placenta prævia (dying on arrival), 1; convulsions—before birth of child, 2; after birth of child, 1; peritonitis following instrumental interference, 1; rupture of uterus, 3. Causes—syphilitic disease (arm presentation, turning), 1; disproportion, 2; pneumonia, 1; syncope and general debility after labour, 1; total, 14.

Forceps.—The forceps I prefer (and that, as a rule, used in the Maternity) is Simpson's long forceps, with the blades both long and strong. Also the handles are longer and roughened instead of being grooved. Of the 93 forceps cases there was one instance of death from subsequent peritonitis. This was after a very severe and prolonged effort at extraction. During my six years in the Cork City Dispensary, and for thirteen years in my private practice, I can only call to recollection one other case in which death of the mother subsequent to the employment of the forceps occurred. Of the 93 cases in which it was applied in our Maternity practice, 7 deaths of the child resulted, though I cannot say whether in some of these the child may not have been dead prior to the use of the instrument; and some of these operations were undertaken in consequence of funis presentation, in which complication the use of the forceps is, in my experience, often preferable to version. As to dilatation of the os, I believe that it is essential to the safety of the mother to wait to secure *fair* dilatation, such as will admit of the application of the instrument, but not so complete as that the lips of the os may not be felt.

Of the advantage of podalic version in contracted pelvis, I have seen many proofs where the forceps has failed, after a reasonable degree of traction and compression, to extract. In two cases in which I was sent for, in consultation, to consider the propriety of

performing craniotomy, delivery was solely effected by this method. In some instances, of course, it is or may be necessary to apply the forceps to the base before we can succeed in delivering the head. In one case I have had to perform craniotomy after version. But the advantage of version in a case of contracted pelvis, with a conjugate diameter of at least three inches, is, I believe, generally admitted—certainly it has been generally adopted by my colleagues in the Maternity with success. Supported as this step is by such authorities as Simpson, Barnes, and others, it commends itself as worthy of serious consideration in all cases where the necessity for craniotomy is contemplated.

Version.—Bi-polar version has been resorted to with success in some cases. The 17 cases in which version was performed recovered, with the one exception of the case of ruptured uterus, the rupture having occurred in a transverse presentation prior to the arrival of the physician, and turning being completed only a few minutes before the death of the woman. The foetus was decomposed, and there was a history of syphilis.

Craniotomy and Evisceration.—In all five cases the mother recovered without any untoward symptom. In all the child was dead prior to the use of the perforator. In two the child was delivered by version subsequently to the reduction of the bulk of the head by the perforator. Evisceration was carried out in two of those formidable cases where the child's arm had descended, and when the hand was for some hours protruding from the vulva, the liquor amnii having escaped for several hours previously, the child being tightly grasped by the uterus, dead, and where there was considerable contraction of the pelvis. A few months since I performed, with Dr. Golding, of Cork, craniotomy on a woman, who had this step carried out in her two previous labours, once at home, and, secondly, in the Lying-in Hospital. She would not consent, though advised, to have premature labour induced. She recovered, without any bad symptom, this, her third craniotomy. The pelvis was considerably contracted at the brim; the child was a male, and delivery was finally accomplished, after reduction of the head, by version. Hysterotomy was performed twice; in one instance the mother died before assistance reached her, from concealed hæmorrhage, accidental, occurring during labour. The child was dead when removed. The other case was attended by Dr. P. J. Cremen, of Cork, when connected with the Maternity, and the particulars reported by him at the South of Ireland Branch

of the British Medical Association. Internal hæmorrhage occurred, followed by faintings; and before it was possible to interfere, convulsions ensued, ending in immediate death. Dr. Cremen at once performed hysterotomy; the womb was full of blood, and contained twins, one of which was alive and breathed for a little time after removal; one placenta was completely detached, the other partially so. The treatment in these truly alarming cases has been debated, but it would appear that the best course under all the circumstances to pursue is—to excite uterine action by such means as rupturing membranes, the use of ergot, and stimulants, at the same time that we secure dilatation of the os by Barnes' dilators, and proceed immediately, when practicable, to turn and deliver the child—the bi-polar method (Leishman) to be always, if possible, selected.

Post partum hæmorrhage.—We have had no death from this cause. Only in one instance am I aware of perchloride of iron having been used to restrain the hæmorrhage—in this instance successfully. The usual means—compression of the uterus, cold, ergot, &c.—have been in all cases sufficient to control even our most alarming cases. Should these means fail, from the satisfactory and encouraging results brought before the profession by Dr. Lombe Atthill of the injection of warm water (at 112°) into the uterus, I would in any future case of severe hæmorrhage be disposed to adopt this plan either in abortion or *post partum* hæmorrhage. I have thus briefly noted a few of the most interesting details of our Maternity practice for the past five years. That they are so meagre I regret, but in a statistical light they may be acceptable to the readers of the Journal.

ART. XI.—*On the Climate of Newfoundland and some of the Diseases chiefly met with there.* By JAMES ALEXANDER, M.D., M.Ch., Dubl.

THE island of Newfoundland, though one of the oldest British colonies, is so little known to most people at home that some observations on the nature of the climate, and the principal diseases encountered during a five years' residence may not be uninteresting. The part of the island where I was stationed is on the N.E. coast, about two hundred miles from St. Johns, the capital, and the settlement of which I had immediate medical supervision is a mining one, at a place called Tiltcove, which has been established

for thirteen years. The population during my residence averaged between five and six hundred, consisting almost entirely of the miners, their wives and families; and, as most of the men were married, the practice was of all kinds.

I consider the climate in that, and indeed in most parts of the island, good, and much healthier than the average English one; for, though the cold in winter is much more severe—the snow covering the ground for about five months—still it is a steady cold, with fine dry, bracing frost nearly all the time, and very few of those sudden vicissitudes so common in an English winter, and which, with the very prevalent dampness, prove so very injurious to those suffering from lung disease. The summers, though shorter, are warmer than those of England, and also dryer, except on the south-east coast, where the vicinity of the Gulf Stream causes a good many fogs, from which the N.E. coast is almost entirely free, foggy days being the exception there. In my opinion a summer voyage to this part of Newfoundland, with residence for a month or two in the warm season, offers many advantages to persons suffering from incipient lung disease, among which may be included the facility of procuring, almost every day, supplies of the fresh liver of the cod, which is often easily taken by persons who cannot digest the oil by itself. I may here mention the case of one gentleman, a friend of my own, who was obliged to give up work in London, suffering from cough, with fine crepitus at the apex of one lung, and considerable falling off in weight, and occasional night sweats; and who, at my suggestion, made the voyage to Newfoundland, and spent some months at the place where I was, with the result of getting perfectly well, gaining largely in flesh, and going home a different man; and he has kept perfectly well since, though back at his old work in London. Anyone going out for health should leave the country by the end of September, as October and November are the worst time of the year, having more rain, with lower temperature. Owing to the very scattered nature of the settlements there are unfortunately no means of getting an accurate death-rate for the island, but I feel certain that, were it compiled, it would compare favourably with that from any part of the world. Serious epidemics are rare; the only severe one that came under my observation was one of a kind of cholera infantum, which occurred one summer at a large settlement called Twillingate, on the opposite side of the Bay from Tilt Cove, and which carried off a number of children—almost all

of whom had not passed the age of teething, and who were attacked, succumbing to it in spite of all the means tried by the medical man of the place, myself, and one whom the Government sent from St. Johns for the occasion—the medicine which we found best, after trials of many, being an emulsion of small quantities of castor oil, with opium and some carminative. At the same place, and co-existent with the epidemic among the children, there were a good many cases of fever among adults, the whole being due, in my opinion, chiefly to the bad sanitary condition of the place—for, unfortunately, the people are very hard to convince on sanitary subjects, and although there are capital laws enacted by the Legislature, they are seldom enforced. The staple industry of the island being the fishery, there is consequently a large amount of offal, especially in the larger settlements, and this is frequently left to rot without any covering of earth or anything else; and I have no doubt that the epidemic above mentioned was due in a great measure to an exceptionally hot season, combined with a very abundant fishery, the place where it occurred being one of the largest settlements in the island.

I would, therefore, recommend any one going out with the intention of remaining a few months for health, to choose one of the mining or smaller settlements in preference to the larger centres of the fishery. At the mines I was able to enforce sanitary measures, and during my residence there, there were no epidemics of any serious zymotic, and none at all except one of whooping cough, and some of a species of influenza that occurred nearly every spring among the children, but which very rarely proved fatal to any of them. There were sporadic cases of fever, and also of erysipelas occasionally; and here I would remark that the fevers I met with were very rarely of the typical typhoid character, corresponding to cases described in text-books, and so often seen in hospital practice; those commonly encountered being of a milder type, with a shorter duration, as a rule, though the temperature was generally quite as high for the first week or ten days; after that it generally fell, also the typhoid spots were frequently absent, and the diarrhœa seldom troublesome, so that I should be disposed to describe the fevers I saw as nearly all cases of modified typhoid or gastric fever, generally tending to recover without dangerous complications, though whether the climate has any effect in this modification or not I am not prepared to say. Typhus occurs occasionally in St. Johns, but is, I believe, unknown in the smaller settlements. Pulmonary

consumption is not unfrequently met with, but I believe that, in the majority of cases, this can be traced to the effects of inter-marriage between near relatives, which, owing to the isolated character of many settlements, is only too common, and, in my opinion, brings many evils in its train, as I consider that hereditary complaints are always aggravated in the offspring of those marriages, and that they are admissible only where both parties are free from hereditary taint. Scrofula in other forms is, of course, met with; also a good many deaths of infants and young children occurring from diseases due to this taint, often aggravated by unsuitable food. The ideas of the majority of the fishing population on the proper food for infants are, like those of most of our own working classes, utterly erroneous; and, besides, in the smaller settlements, even with the will, the way is often wanting, owing to the impossibility of procuring milk, which is so essential to the proper nourishment of infants, when, from any cause, they are deprived of the natural supply from the mother. I have over and over seen children being fed with nothing but a watery solution of corn flour or some other starchy compound, with a little sugar; but, as I have also seen that these children manage to exist and grow on it, though, of course, not thriving as they would on more suitable food, I cannot agree with a statement which I have seen emphatically recorded by several authorities that starch is wholly indigestible by infants before the time of teething begins. The chemical argument is, no doubt, good, but the facts are against it, though, of course, I would never recommend food of this sort for infants, and always made it be changed when I came across it in practice. Of diseases affecting the respiratory organs, I found the commonest to be pneumonia—the type of this I most frequently met not being very acute, and seldom affecting both lungs. In many cases it seemed to come on insidiously, beginning with symptoms of a common cold, and without well-marked rigors, and the lung affected often becoming consolidated without much constitutional disturbance, except a high temperature, as indicated by the thermometer. With regard to the temperature in these cases, I noticed sometimes a curious fact, that it was occasionally higher on the affected side. I remember one case in particular, when, on taking the temperature one morning, I found it 107° , and this without any particular distress. I then put the thermometer in the axilla of the sound side, where it registered only 103° . Thinking there might have been some mistake, I carefully repeated both

observations, and with the same results; and in several other cases I have noticed a difference of from a half to one and a half degrees between the affected and sound sides. I have never seen this mentioned in any text-book, and I think the subject worth further investigation. The thermometer used was a reliable one, made by Harvey and Reynolds, of Leeds.

In these cases of pneumonia in which the lung dulness came on so quietly, I found it took a long time to get rid of it, the dulness often remaining for two or three weeks after the patient was apparently quite well, and giving rise to the necessity for careful watching. Bronchitis was the next in order of frequency, and here again the cases were commonly of a mild type, and generally got well soon. Pleurisy I sometimes found along with the pneumonia cases, but rarely *per se*; and I had no cases of extensive effusion or empyema, and though I think few, if any, cases of pleurisy recover without some effusion, still in those I met it was generally slight and soon got rid of. I think the generally mild character of these diseases is due partly to the good air, and partly to the healthy open-air life led by most people in the colony. Almost every spring, about the time that the snow is melting and passing away, an epidemic of influenza occurred among the children in the settlement, and I have ascertained that this is common all over the island; it is a sort of thing that keeps a medical man busy for a time, but, fortunately, rarely proves fatal, only a young and badly-nourished infant occasionally dying under it. There is one complaint from which I believe almost every child in the country, and also many adults, suffer at one time or another—viz., round worms, the most common being the *ascaris lumbricoides*, and in children they often occasioned high fever, and sometimes convulsions; I have frequently known as many as a dozen, and sometimes even twenty large round worms got rid of by a small child after a dose of santonine. I always prescribed this, and have given many hundred doses to children of all ages, but have never seen any troublesome effects from it. I am unable to say why this complaint should be so general, the children of the rich and poor alike suffering from it, and though I have frequently spoken to my brother practitioners in the colony about it, I have never heard any valid reasons assigned for its extraordinary prevalence, though they all assent to the fact.

Another very common complaint is dyspepsia in its many forms; this at first sight would seem strange, as the population are almost

entirely engaged in out-door pursuits, and lead an active life, and are, as a rule, a hardy race; but the cause I think can be found in the enormous quantities of tea consumed, and especially in the fact that it is generally drunk at a very high temperature. I think there can be no doubt that the habit of drinking fluids either very hot or very cold is a fruitful cause of dyspepsia, such fluids acting as stimulants to the mucous membrane of the stomach, and when often applied bringing it into a state of chronic congestion—the prevalence of dyspeptic disorders among our American cousins, who imbibe such quantities of iced water, being a further confirmation of this fact.

These are the principal remarks I have to offer; and I would say, in conclusion, that any one going out to Newfoundland for a summer voyage will find it most enjoyable, and will meet with a hearty hospitality from all ranks of society there—and, if a sporting man, will be fully able to gratify his tastes in that respect.

TREATMENT OF DIABETES MELLITUS WITH SALICYLATE OF SODA.

RYBA and Plumert have arrived at the following results:—1. Salicylate of soda, given in daily doses of eight grammes, determined a decided diminution of saccharine excretion. 2. Considerable differences are observed, according to the severity of the case. In recent cases the sugar can be made to disappear completely, and does not reappear immediately after the remedy is stopped. In cases of longer duration, the effectiveness of the remedy is also apparent, though the sugar does not disappear entirely, nor is there a favourable after-effect. Lastly, in cases of several years' duration, with severe diabetic symptoms, no result was attained by treatment, at least not from the small doses administered. 3. The diminution of saccharine production is more remarkable by the greatest restriction of hydrocarbons in the diet. 4. Diminution of the quantity of urine is parallel with the diminution of sugar; but in one case the polyuria remained in spite of the considerable diminution of saccharine excretion. Other diabetic symptoms, and the bodily weight, are also favourably influenced. 5. In two cases the quantity of both urine and sugar was increased for a short time after the commencement of medication, followed, however, by progressive diminution. Treatment by salicylate of soda is followed by a greater diminution of sugar excreted than when the treatment is indifferent.—*Präger Med. Woch.* and *N. Y. Med. Jour.*, Feb. 1878.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

RECENT WORKS ON OPHTHALMOLOGY.

1. *A Manual of Ophthalmic Surgery.* By B. THOMPSON LOWNE, F.R.C.S., Eng., &c. Smith, Elder, and Co. 8vo., pp. 194. 1876.

It is very difficult to conceive with what object this work was written and published, for it cannot possibly further the science of ophthalmic surgery, to the study of which, we are informed in the preface, it is intended as an introduction; nor can it in any way add to the reputation of the author. If this be taken as the type of books intended "to supply the student and general surgeon with a short and concise account of the various diseases of, and operations on, the eye, and with just as much information on the subject as every practitioner should possess" (we quote from the preface), we can only say such books are dangerous works for students and general surgeons to get hold of, for if they do not propagate inaccuracy they at least foster natural laziness. Ophthalmology is altogether too important a subject to be "made-up" by such unreliable smattering as the present work contains; and it is greatly to be deplored that a respectable publishing firm should allow such intolerable trash to go forth stamped with their name, for it cannot fail to bring discredit upon the English school of ophthalmology. That this is no hasty or unjust judgment may be easily seen from the following specimens taken from different portions of the book:

At page 25 we find this description of a well-known condition of the conjunctiva:—"Pinguecula.—Two or three small lobules of fat, sometimes form between the cornea and the inner canthus."

Pinguecula is not a lobule of fat, nor does fat form any part of it. It is due to a thickening of the conjunctival and subconjunctival tissue. The author makes the not uncommon mistake of using the term epiphora for stillicidium. The former is properly applicable alone to lacrymation depending on hypersecretion, the latter to lacrymation caused by obstruction of the lacrymal passages. We are quite of the opinion that there are altogether too many

useless terms in ophthalmology; yet, when such terms exist and are in use, they should at least be applied in their proper sense.

In the two chapters—V. and VI.—devoted to refraction, the author succeeds, on the whole, in keeping clear of blunders. (One glaring misstatement, however, occurs at p. 50, which raises the serious doubt in our mind as to whether the author really understands what he is writing about. He says, speaking of the choice of glasses for a presbyopic patient:—"If a person prefers to look through convex spectacles a little removed from the eyes, they are too strong."

Exactly the opposite is the fact, as must be apparent to any one who understands the action of a convex lens.

In all the examples given, the old system of numbering glasses is adhered to. This, we presume, is to be accounted for by the statement we find in Chapter VII., that though "efforts are being made to adopt the metre as a standard of measurement, . . . it will probably be long before it is accomplished." Even in this city, where we are not remarkable for quickly adopting changes, the metrical system of notation in glasses has been in use for some time.

Speaking of diplopia, not a word is said of the method of determining whether it is homonymous or crossed diplopia that the surgeon may have to deal with. We should have thought that the author might have, at least, accorded "just as much information on the subject as every practitioner should possess," especially considering its great importance.

He has no fears about the removal of an eye affected with panophthalmitis—"experience does not show that there is any danger in doing so." Before publishing a second edition of his book, we should recommend him to peruse the literature on this subject.

The last passage we shall quote exhibits, perhaps, more downright ignorance than any other portion of this really worthless manual. It purports to be a description of what "is known as Graefe's linear extraction." We distinctly assert that Graefe never proposed such an operation as is here described. In passing, we may note that, according to Mr. Lowne, "chloroform should always be administered." We hold exactly the opposite opinion, but do not, of course, pause to discuss the question as, after all, it is principally one of opinion.

A stop speculum having been introduced and the globe fixed in the usual way, the operator "inserts a broad needle half a line behind the sclero-corneal junction." We confess that this

completely baffles us. Who ever heard before of a "broad needle" being used for Graefe's operation, or that Graefe recommended his incision to be made through the ciliary body, as a puncture "half a line behind the sclero-corneal junction" implies? We are compelled to admit our inability to explain how a gentleman holding the post of Arris and Gale Lecturer on Anatomy and Physiology at the Royal College of Surgeons, could have gravely penned such a description. It is quite unnecessary to criticise the other steps of the operation as described by Mr. Lowne; but we may just notice one amusing statement he makes at the conclusion of the paragraph which he has devoted to this subject. He says:—"The surgeon must be able to use the knife (broad needle?) with his left hand when the left eye is operated on, as the cornea can only be entered from its outer side." True, if he always stands behind the patient when operating. But what objection can there be to his placing himself at the patient's left side when operating on the left eye and using his right hand? If we are not mistaken Graefe always did so.

We conceive we have brought forward quite a sufficient number of examples to prove the justness of our verdict with regard to this manual of blunders. Indeed, it may possibly appear to many of our readers that too much time and space has been devoted to a very unworthy object. But the truth is, we are fallen upon perilous times, for not alone in this particular department of ophthalmology, but in almost every department of science, there is an ever-increasing noxious crop of these so-called manuals and hand-books springing up, the chief characteristics of which are, either crude notions hastily and badly put together, or utter unreliability, as in the present case. The real fault lies in the present system of examinations, which naturally begets and nurtures everything in the shape of "cram." "Short studies on great subjects" are, we suppose, inevitable, while the present system lasts. If so, it is not too exacting to require that at least they should be accurate.

2. *How to Use the Ophthalmoscope: being Elementary Instructions in Ophthalmoscopy, arranged for the Use of Students.* By EDGAR A. BROWNE, Surgeon to the Liverpool Eye and Ear Infirmary, &c. Trübner and Co. 1876. 8vo., pp. 108.

THIS little work cannot be spoken of too highly. It is compiled by one who has evidently had practical experience in the arduous

task of teaching the ophthalmoscope to students, and who has thoroughly recognised the difficulties they labour under in acquiring the necessary dexterity in its use. In the preface, remarkable for its modesty, the author apologises for having apparently treated his subject in too elementary a manner; but he says:—"I have deliberately done so, as experience in *vivâ voce* teaching has shown me that a considerable proportion of students who commence the study of ophthalmoscopy are not prepared to receive more than the most rudimentary information. To this, if sound, details can always be added." This opinion will be thoroughly endorsed by everyone who has had any experience of teaching medical students, at all events in this country. The true explanation of this state of affairs is, the total absence of a proper and thorough preliminary education; and we are compelled to admit that, as a rule, the students from the universities display no greater proficiency in elementary science than those who have never experienced the advantages offered by a university. Is it not the exception, even in a large class, to meet with a single individual who has the faintest notion of the laws of refraction, or who could tell what a lens is, or explain the action of a prism?

The arrangement of this little work is admirable, and leaves nothing to be desired. The author's scheme is, as he says in the preface, "strictly limited to counselling the student how to acquire the art of seeing and the power of recognising and localising the commonest deviations from the normal standard in the individual structures."

The first section contains a capital epitome of the information every student ought to possess of the rudiments of optics, especially as regards the action of lenses, and the optical principles involved in the construction of the ophthalmoscope. Section II. is devoted to the description of the instrument, together with some excellent hints regarding the method of using it. Sections III. and IV. treat of the appearances of healthy structures and disease respectively. The latter sections are, of necessity, sketchy, but the subject matter is so accurately and, at the same time, so simply put together, as irresistibly to arrest the attention of the student. We cordially recommend this little book as a valuable source of information and reliable guide to the student commencing the study of ophthalmoscopy.*

3. *Hints on Ophthalmic Out-Patient Practice.* By CHARLES HIGGENS, F.R.C.S., &c. J. & A. Churchill. Small 8vo., pp. 87. 1877.

A TREATISE on ophthalmology in 87 pages, which ought to excite a feeling of ecstasy in the minds of those who delight in what they are pleased to call the "practical school" of medicine—a term applied, *par excellence*, to the Dublin school. Truly, this is science at high pressure with a vengeance, yet we cannot resist a strong feeling of doubt which arises in our mind as to whether this and kindred "primers" should not be vigorously discouraged. If ophthalmology is worth knowing it is worth knowing well, and the tendency of superficial works like the present is, as we said above, to foster natural laziness, and to degrade, in place of elevating, the science they purport to inculcate. It is but fair to state, however, that the little book before us contains a marvellous amount of very accurate and useful information.

Medico-Chirurgical Transactions. Second Series. Volume the Forty-second. London: Longmans, Green, Reader, and Dyer. 1877. 8vo. Pp. 335.

THIS volume fully maintains the prestige of the Royal Medical and Chirurgical Society of London. It contains twenty-three papers, most of them of a high order of merit, on subjects connected with practice of medicine, surgery, and pathological anatomy. The work is published in their usual first-rate style by Messrs. Longmans' firm.

Hints on Insanity and Signing Certificates. By JOHN MILLAR, L.R.C.P.; ex-Medical Superintendent, Bethnal House Asylum, London. Second Edition Enlarged. London: Henry Renshaw. 1877. Pp. 141.

BUSY practitioners, who, in the course of their daily work must unavoidably meet with cases of mental derangement, and who, "running as they read," are necessarily unable to study larger works devoted to such a special subject, will find many useful and practical hints, both from the diagnostic, as well as from the therapeutic points of view, on the different forms of mental alienation in Dr. Millar's little work, which has now reached a second and enlarged edition. The information contained in it is concise and well

arranged, and the appendix presents all the necessary forms, &c., for certifying for patients, and the different modes of procedure for the "admission" and "discharge" of those suffering from insanity according to the English lunacy system. We can strongly recommend the perusal of this little volume to the general practitioner.

RECENT WORKS ON ELECTRICITY.

1. *The Electric Bath: its Medical Uses, Effects, and Appliance.* By G. M. SCHWEIG, M.D. New York. 1877. Pp. 134.

DR. SCHWEIG'S unpretentious little work will be of use in calling attention to a method of treatment which has hitherto been left almost entirely in the hands of enthusiastic specialists or ignorant charlatans. As yet no satisfactory evidence has been adduced of the superiority of the bath over methodical localised applications of electricity, and Dr. Tibbits, who has made careful comparative experiments within the last few years, concludes that, except in certain gouty and rheumatic cases, its action is less beneficial than that of localised electrification.

2. *How to Use a Galvanic Battery in Medicine and Surgery.* By HERBERT TIBBITS, M.D. Pp. 56.
3. *A Handbook of Medical and Surgical Electricity.* By HERBERT TIBBITS, M.D. Second Edition. London: J. A. Churchill. 1877. Pp. 247.

As the substance of the pamphlet is embodied in the larger work, our remarks will apply to the latter, which we may characterise as being, in the main, a serviceable practical guide how and when to employ electricity in medicine and surgery.

This edition is increased in size by more than one hundred pages, and contains thirty-one additional engravings, among which we observe reproductions of the useful drawings of the motor points and lines from Ziemssen's work. The directions for the management of the batteries and other electro-medical apparatus are fully and clearly put, and are evidently the result of long personal experience. If a third edition be called for, we would suggest two emendations in the opening chapter. One is to omit the tedious descriptions of Duchenne's cumbrous and costly instruments, which have now little more than a historical interest; the other is to leave

out the prefix which ostentatiously heads the description of each of the instruments (the Tibbits Voltaic Battery—the Tibbits Combined Hospital Battery, &c.) recommended by the author, and which, although doubtless excellent, do not present any special novelties in construction requiring a distinctive designation. Nearly half of the entire work is devoted to a fair and readable account of the leading applications of electricity in medicine, concluding with a summary of the relations of electricity to surgery, and to midwifery and diseases of women.

Lectures on the Diseases of the Nervous System. By PROFESSOR J. M. CHARCOT. Translated by G. SIGERSON, M.D. London: the New Sydenham Society. 1877.

IT is almost needless to call attention to this volume which, under the auspices of the New Sydenham Society, is introduced to a wide circle of readers, for its own intrinsic merits will assuredly place it at once in the foremost ranks of medical literature. M. Charcot is not only a most accomplished physician but is also a charming writer, and the profession can now freely profit by the record of the observations and experience of a great teacher in a sphere of pathology in which he has had exceptional advantages. Each lecture is full of interest and value, and it will suffice, without adding a word of criticism, to indicate the subjects discussed by the author. Part I., embracing four lectures, treats in a most instructive manner, of the nutritive disorders consecutive to lesions of nerve-trunks, of the spinal cord, and of the brain respectively. Part II. includes the remaining nine lectures; and, after an admirable account of the symptomatology of paralysis agitans and disseminated sclerosis—two affections which were long confounded—the concluding lectures deal with special manifestations of hysteria, in all the forms of which the Parisian hospitals are so rich. In this part of the book we have a study of some of the most curious and perplexing cases that confront the physician, and we owe much to M. Charcot for the help he has rendered in the unravelling of these singular phenomena.

PART III.

HALF-YEARLY REPORTS

REPORT ON NERVOUS AND MENTAL DISEASE.^a

By RINGROSE ATKINS, M.A., M.D., &c.; Resident Medical Superintendent, District Lunatic Asylum, Waterford.

I. GENERAL CONSIDERATIONS OF INSANITY.

New Classification of Mental Diseases.—Dr. W. H. O. Sankey, in the *Journal of Psychological Medicine*, April, 1877, proposes a new system of classification, which he states, after twelve years' experience in teaching, he has found to be most convenient. It has in view the arrangement of the cases usually met with in asylums into groups according to the nature of the maladies. Taking then a general survey of all such cases, they may, he says, be separated into two great divisions:—

(A.) Those in which the mental symptoms are primary, or essential, or idiopathic.

(B.) Those in which they are secondary or *symptomatic*.

(A.) may be subdivided into—(a) in which the cases depend upon a purely pathological change, or what is really disease, and (b) in which the mental symptoms are due rather to *developmental* causes.

In (a) the author as yet recognises but two *species morbi*—(1) Ordinary Insanity; (2) General Paresis.

In the subdivision (b) there are also two kinds of cases—(1) in which the developmental anomaly is an original conformation, or idiocy; (2) in which it is due to decay or senile imbecility.

The division (B) in which the mental phenomena are only

^a The author of this Report, desirous that no contribution to the subject of Nervous and Mental Disease should remain unnoticed, will be glad to receive any publications which treat of it. If sent to the correspondents of the Journal, they will be forwarded.

symptomatic, or accidents in the cases, forms perhaps a smaller moiety of the inmates of asylums, and the most frequently occurring kind of cases are the four following:—(1) Epileptic insanity; (2) Alcoholismus; (3) Spinal disease extending to the cerebral regions; Organic disease of brain as the result of apoplexy, &c.

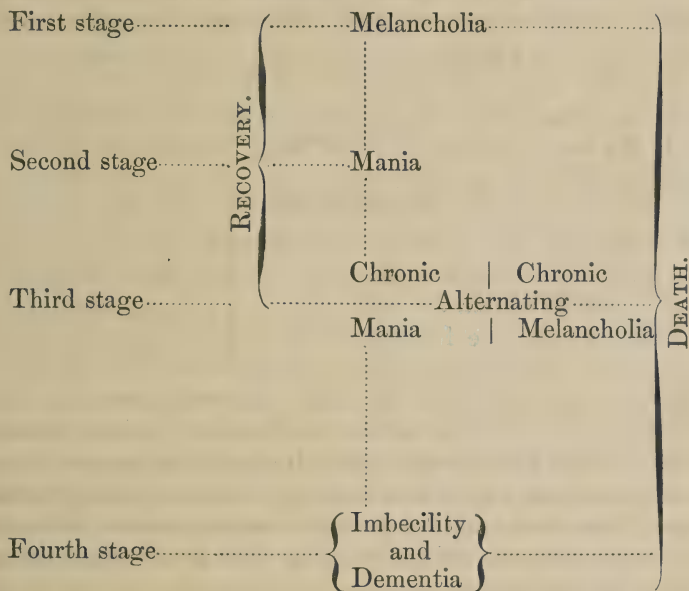
Tabulated this classification then appears thus:—

(A.) Idiopathic	{	1. Pathological	{ 1. Insanity proper.
			{ 2. General paresis.
(B.) Symptomatic	{	2. Developmental	{ 3. Idiocy.
			{ 4. Senility.
	{		{ 5. Epileptic insanity.
			{ 6. Alcoholismus.
			{ 7. Spinal Diseases.
			{ 8. Organic cases.

This corresponds very closely with the result arrived at by the International Committee for the classification of mental diseases at Paris. “In this classification, including only two species of actual mental disease, it will be found that many of the almost innumerable varieties described in text-books—such as mania, melancholia, &c.—are as yet quite unaccounted for. But many of these so-called diseases are but symptoms, and often but passing symptoms in the course of a disease; and many of the phenomena thus promoted to the position of a distinct disease, not only are transient, but succeed each other in the progress of a single case.” A case of ordinary insanity at its first outset commences by a stage of melancholy, which is at first considered as acute melancholy. The case has in this stage four modes of termination—(1) in recovery; (2) in death; (3) in passing into mania; (4) in becoming chronic (chronic melancholia). When in its evolution it becomes mania, it has been falsely called a new disease. This stage too, or acute mania, has various modes of termination—(1) in cure; (2) in death; and (3) in chronicity; in the last category it has received various names—as chronic mania, delusional insanity, incoherence, &c. It often occurs that when the disease has become chronic, the violence of the mania abates, and so after a time do the melancholic symptoms of chronic melancholia; thus the case becomes one of chronic insanity, probably with delusions, incoherence, &c. In this both recovery and death are less common, and death would probably be due to some accidental cause rather than the disease itself.

The following diagram will represent the stages and progress which a case of ordinary insanity may take:—

Diagram of the Evolution of Insanity proper.



If the whole sum of mental disease is thus reducible to two, it will, perhaps, be asked what is the nature of these cases to which the familiar terms puerperal mania, phthisical mania, oinomania, or dipsomania, suicidal mania, homicidal mania, &c., &c., have been applied. There is nothing in any of these, the author considers, which should constitute them a distinct *species morbi*. All these varieties, which are merely ordinary symptoms of disease, occur at times in the course of a primary attack—as the suicidal propensity, in an attack of acute insanity in the melancholic stage; nymphomania also in the early stage; while cleptomania is a common symptom in the early stage of general paresis. Dr. Sankey does not wish to maintain that no other species of insanity may not yet be eliminated; but at present, as far as his experience and examination have extended, only two have been actually proved.

On Statistical Tables of the Causes of Insanity.—In the October number of the same journal, Dr. Herbert C. Major has an interesting paper on the above subject. He argues that the etiological tables published from year to year in nearly all

Of course, if desired, the subdivision into moral and physical causes can, under this system, be made in the usual way, and probably the table would derive additional value by the *percentage value* of each causative agency being given. These are matters of detail; the essential point being that of no single cause is it stated that it has produced a given number of cases, but that it has been *concerned in their production*.

Katatonía, a Clinical Form of Insanity.—In the *American Journal of Insanity* for July, 1877, is a paper by Dr. James G. Kiernan, of the New York City Lunatic Asylum, bearing this title. The author states at the outset that to the observations of Dr. Kahlbaum, Medical Superintendent of the Private Asylum at Gorlitz, Prussia, is due the recognition and clinical demarcation of this form of mental disorder. Dr. Kahlbaum claims that its distinguishing characteristic is an irregularity—or, as he phrases it, an insanity—of tension, mental and muscular, whence the name *Katatonía*. Its prominent symptoms, in addition to that of change of temper noticed during the inception of other forms of insanity, are the occurrence at times of marked rhythmical motions, always under the control of the will, its cyclical character, maniacal, melancholic, and cataleptoidal conditions, alternating with more or less imperfect convulsive attacks; there are also pathetic delusions of grandeur, and a tendency to talk and act theatrically. Erotic manifestations of some kind frequently occur, and, as is usual under such circumstances, the patient's ideas have a religious tinge. At any stage, as in other nervous diseases, remissions, or, as is claimed by Kahlbaum—though, Dr. Kiernan thinks, erroneously—complete recovery may ensue. If the case is to end unfavourably periods of excitement and stupidity recur with more and more frequency, and the patient dies with terminal dementia. Dr. Kiernan records the histories, with *post-mortem* appearances—gross and microscopical—in a number of cases, and enters upon the questions of causation and treatment—which space, however, prevents me reproducing here.

II. ANATOMY AND PHYSIOLOGY OF THE NERVOUS SYSTEM.

The White Substance of the Spinal Cord.—"The Conducting Tracts in the Brain and Spinal Cord of Man, on the Basis of Embryological Research," by Dr. P. Flechsig. On the strength of embryological observations the author of this treatise divides the white substance of the spinal cord into the following territories:—I. *The anterior pyramidal tract*—a narrow

zone, limiting the anterior fissure. II. *The main anterior fasciculus*—identical with the anterior column, with the exception of the previously mentioned zone. III. *Goll's columns*—the narrow triangular region on each side of the posterior fissure, also known previously as the funiculus gracilis, but merely in the cervical part of the cord. IV. *Burdach's columns*—occupying the remainder of the posterior columns, and continuous with the fasciculus cuneatus in the medulla. V. *The direct lateral cerebellar tract*—a narrow zone at the periphery of the lateral columns, extending from near the exit of the posterior roots to about the centre of the periphery of the lateral columns. VI. *The lateral crossed pyramidal tract*—extending inwardly from V. to the posterior horns of the grey substance. VII. *The remainder of the lateral column*—which he subdivides into (a.) the lateral border zone of the grey substance, a narrow zone surrounding the lateral boundary of the grey substance, and (b.) *the anterior mixed region of the lateral column*, the remaining (anterior external) part of the lateral column.

In the fully developed cord these separate tracts are not visibly isolated; but, as Flechsig maintains, they may be recognised by the size of their nerve fibres. By dividing nerve fibres into four classes, according to their thickness, he finds that each region differs slightly from the neighbouring territory in the size of the nerve fibres constituting the bundle. But more interest centres in the fact that the course of the separate system of fibres traced by means of the embryological method is identical with the course assumed by the so-called *secondary degenerations* of Türck. Flechsig's results on the course of the tracts may be summed up as follows :—

The Pyramidal Tracts.—The fibres of each pyramid of the medulla oblongata ordinarily follow two different roads in the spinal cord—viz., the anterior pyramidal tract of the same side, and the lateral (crossed) pyramidal tract of the opposite side. Considerable variations are found, however, in the numerical distribution of the pyramidal fibres in these tracts of the cord, not only in different individuals, but even in the two lateral halves of the cord. While *semi-decussation of both pyramids* has been most frequently found, Flechsig has also seen partial decussation of only one pyramid, with total crossing of the fibres of the other to the opposite side, and even *total decussation of both pyramids*, with entire absence of both anterior pyramidal tracts. The other extreme—total absence of decussation—he has not yet met with, although

occasionally but very few fibres did cross to the opposite side. Since the pyramids are now recognised as the tracts of voluntary impulses, this variability in the connexion of one side of the brain with the same or opposite side of the cord is of great clinical interest. The total area of the pyramidal tracts diminishes as they descend in the cord. The lateral pyramidal tract disappears usually about the level of the third or fourth sacral nerve, its fibres having gradually entered the grey substance. The anterior pyramidal tract disappears at the upper limit of the dorsal part of the cord sooner or later, according to its original volume; the termination of its fibres is not settled. A system of fibres running from the nucleus lenticularis and internal capsule, through the pes pedunculi and pons, into the pyramid of the same side, and thence down into the spinal cord, is the tract of *Türk's descending degeneration*. Any lesion of this tract is followed by degeneration of the fibres below the seat of the lesion. As far as Flechsig's researches go, this system of fibres is identical with his pyramidal tracts. [In a recent note in the *Centralbl. f. d. Med. Wissensch*, 1877, No. 3, Flechsig states that he has since traced the pyramidal fibres as a compact bundle through the pes pedunculi and internal capsule into the centrum semiovale, in the vicinity of the sulcus centralis.] The variability in their topographical distribution in the cord sheds a new light on the previously found differences in the locality of the degenerated fibres in the cord.

The direct Lateral Cerebellar Tracts.—Fibres radiating from the grey substance, especially in the columns of Clarke, collect in a flattened bundle, occupying a narrow zone at the periphery of the posterior half of the lateral column, which bundle is to be found as low as the lumbar part of the cord. Other fibres, however, from the same source, ascend the cord intermingled with the remaining fibres of the lateral column, without forming a distinct bundle; they are shown to be equivalent to the first-mentioned fibres by the time of their development. Increasing in number as they ascend, both sets of fibres enter the corpus restiforme in the medulla; and as the *stratum zonale* gains its posterior part, Flechsig has traced them no further than the cerebellum, where they apparently run towards the vermis superior. In the *ascending form* of secondary degeneration, this system of fibres is one of the tracts involved, any lesion in its course being followed by degeneration of the fibres above the lesion. *The remainder of the lateral columns* consist of fibres radiating into it from the grey substance. The lateral border

zone of the grey matter receives besides fibres coming from the posterior roots, while fibres from the anterior roots enter the anterior mixed region of the lateral column. On this account the area of the lateral columns enlarges considerably at the places where the roots of the spinal nerves contain most fibres—viz., at the cervical and lumbar enlargements of the cord. The fibres of these tracts enter the formatio reticulans of the medulla oblongata. *The brain anterior fasciculus* receives fibres both from the anterior roots of the opposite half of the cord (through the anterior commissure), and from the anterior horns of the grey substance of both sides. It is continuous with the fasciculus longitudinalis posterior of the medulla.

Goll's columns are distinctly isolated in embryological specimens only as far as the upper third of the dorsal region, but very probably continue to the lumbar enlargement. Their constituent fibres are furnished partly by the columns of Clarke of the same side of the cord, partly by the posterior horn of the opposite side, by route of the posterior commissure. Increasing in area as they ascend, they apparently terminate in the "nuclei of the funiculi graciles" in the medulla. This system of fibres (together with the lateral cerebellar tract) is the seat of the ascending form of secondary degeneration.

Burdach's columns are composed largely of fibres from the posterior roots, and hence show the greatest area at the enlargements of the cord, where the spinal roots are the most voluminous. But fibres composing these columns can also be traced into the grey substance radiating towards the anterior horn, the posterior commissure, and the columns of Clarke. The tract terminates apparently in the "nuclei of the funiculi cuneati" in the medulla.—(*American Journal of Nervous and Mental Diseases*).

Cerebral Localisation.—A note by M. Maurice Reynaud was offered at the Acad. de Médecine of Paris, Dec. 5, 1876 (Rep. in *Gaz. des Hôpitaux*), called out by the memoir of Dr. Proust on "Cerebral Localisation," in which he gave the history of a consumptive patient in whom, three days previous to his death, there appeared suddenly a paralysis limited to the left superior extremity, and affecting exclusively the hand and forearm. At the autopsy the only lesion discovered was a very minute patch of red softening developed in the right hemisphere around a meningeal tubercle. This patch, which was only as large as a twenty centime piece, was situated in the ascending parietal convolution, and in the grey

substance forming the base of the fissure of Rolando, five centimetres from the superior internal border of the hemisphere. This point is precisely the one that has been experimentally demonstrated to be, in the ape, the centre in relation with the movements of the inferior member. This observation is the only one, in M. Reynaud's knowledge, in which the brachial paralysis, to the exclusion of every other cerebral phenomenon, permits the establishment of a certain convolution between the functional trouble and the anatomical lesion. [A very similar case, both as regards the localisation of the paralysis and the seat of the cerebral lesion, though differing as to its cause, was recorded by me in a series of cases communicated to the British Medical Association, at its Manchester meeting.—*Rep.*] ^a

At the meeting of the Société de Biologie, Feb. 10, 1877, M. Duret, following M. Onimus, said that there were three methods of experimentation available for demonstrating the existence of cerebral centres:—1. Electrification. 2. Ablation of parts of which we wish to learn the physiological functions. 3. Clinical and surgical observations. The electrification experiments are exceedingly difficult to interpret on account of the diffusion of electricity; there are, in fact, regions that do not respond to electricity; and he asked a physiological explanation of the fact. He did not think, with M. Onimus, that the conduction of the vessels sufficed to explain certain phenomena of electrification that were observed. The experiments by ablation, on the other hand, gave us to-day positive facts and results perfectly clear and well-defined.

M. Onimus said he had never seen any permanent paralysis by the mere ablation of a part of the brain in animals.

M. Charcot observed that certain pathological facts, too, well studied, and already very numerous, also were in favour of localisations, and that we cannot counterbalance them by experiments upon animals.

M. Laborde asked to be allowed to add a fourth to the three

^a In the Med. Examiner for March 14, is a note of a case recorded by Prof. Hardy, of Paris, in which the following symptoms were observed:—Great feebleness in right arm, where sensibility remains unaltered; no change of temperature; pulse hard, and indicating atheroma; arcus senilis slightly marked; sudden death in eight days. On *post mortem* examination, in addition to slight atheroma of the arteries, at the lower two-thirds of the fissure of Rolando, on a level with the ascending frontal and ascending parietal convolutions, was found a plate of miliary granulations, so closely massed together as to form a species of lining membrane. They were not, however, incorporated with the pia mater itself, which was interposed between them and the cerebral substance—in a word, the pia mater and nervous tissue were intact.

methods mentioned by M. Duret for the study of cerebral localisations—one that he had attempted to introduce into physiological experimentation—he referred to the artificial production in the brain of the dog of hæmorrhages exactly similar to those observed in man. He recalled the fact that he had communicated to the Society the results of these experiments, and that he had obtained by this procedure symptoms exactly like those observed in man in cases of cerebral hæmorrhages. These facts demonstrate that there are in the brain of the dog limited regions, the excitation of which produces perfectly localised phenomena.

M. Lepine remarked that M. Laborde's method was a great advance in physiological experimentation, and that it was much to be preferred to that of Goltz, who had destroyed portions of the brain by a jet of water at high pressure introduced through a small orifice in the skull.—(*Am. Journ. Nerv. & Ment. Dis.*)

III.—CEREBRO-MENTAL PATHOLOGY AND HISTOLOGY.

Aphasia.—The following are the conclusions of a lengthy memoir by Dr. Augusta Tamburini concluded in the issue of the *Rivista Sperimentale di Freniatria e di Medicina Legale* for Sept. and Dec., 1876 (translated in the *Am. Journal of Nerv. and Ment. Dis.*)

1. The faculty of language is a complex function constituted of various elements, isolated in their pathological lesions, which may be put in relief by clinical analysis.

2. Anatomico-pathological researches, based on a large number of cases and supported by physiological data, have been able to localise each of the functional moments of language.

3. The memory of verbal images and their co-ordination, which constitute the more intellectual part of language, have their seat in the grey matter of the cerebral hemispheres, and more particularly in the frontal lobes.

4. The transformation of sensory (optic, acoustic, &c.) impressions and their verbal reflection in a motor impulse towards the extrinsic muscles, has its localisation in the marginal convolutions of the Sylvian fissure, and more especially in the third frontal convolution.

5. The route followed by the motor impulse to join the nerves animating the muscles of speech, runs from the convolutions of the insula (which are only the continuation of the marginal ones), by way of the nucleus lenticularis and the corpus striatum, traverses the cerebral peduncles (*crura cerebri*) to the protuberance (*pons*)

Varolii) and the medulla oblongata, in which are found the nuclei of the muscles of phonation.

6. The motor impulse sent to the phonetic muscles is co-ordinated from its point of origin, and it is not needful to look for co-ordinating centres along its course.

7. Neither physiology nor pathology authorises us to retain the olivary bodies as the special co-ordinating centre for speech. To it and to the bulbar system in general can be attributed the bi-lateral harmonisation of speech, as of every other voluntary movement.

8. The lesion of any one of the points in which are located the mnemonic sonorous images of words, in which, thus kept, they are transformed into motor excitations, and from which they are transmitted as centrifugal impulses to the executive muscles of speech, may produce aphasia of ideation, of transformation, or of execution; and secondly, according as the seat of this lesion is more or less central, the functional disturbance may be more pronounced in the intellectual or in the peripheral phenomena of speech.

9. The facts adduced against the functional localisation of speech are explained either by the unilateral predominance of this function or by the laws of cerebral compensation and of irritative irradiation.

10. Aphasia, although met with most frequently as a symptom, may, nevertheless, be studied as a malady of itself taken in the sense, and in cases of simple alteration, of one of the elements constituting the function of speech.

Disturbances of Sensibility in Ataxies.—The following are the results of a series of observations on the disorders of sensibility in ataxies made at the Salpêtrière by M. P. Oulmont, and reported by him to the Soc. de Biologie, Feb. 17 (*Gaz. des Hôpitaux*). His procedure was to make a figure of the patient, and to mark on these with coloured crayons the anæsthetic patches. From these designs the comparison of about twenty unselected ones, taken as they happened, permitted the deduction of the following conclusions:

1. The disorders of sensibility (that to pain is here alone considered) are nearly constant.

2. They are scattered over the entire body; the head even being implicated in a majority of cases.

3. They are generally disposed with a kind of symmetry, especially manifested on the body and limbs.

4. They occupy in each portion of the body certain points of election. On the head, these are the cheeks and supraorbital regions, while the neck is almost invariably free. On the trunk,

they are the bosom, the omoplates, and lumbar region, while the sternal region is more frequently intact. In the superior as well as in the inferior members, the lesions of sensibility are more extended and more advanced towards the periphery than nearer the centres. The islets of healthy skin occupy, in preference, the fold of the elbow, and the palm of the hand in the upper extremity; and the internal aspect of the thigh, in the inferior members. This division of the anæsthetic patches seems characteristic of tabes dorsalis, and M. Oulmont had been able to make it useful in two doubtful cases in confirming the diagnosis.

Psychological Pathology of Progressive Paresis.—Dr. Edward C. Spitzka, of New York (who recently obtained the W. and S. Tuke Prize of the British Med. Psychol. Association for his Essay—"The Brain is always the seat of Insanity, but it is not always the seat of its Cause"), writing on this subject, thus says of its morbid histology:—"My results are mainly in accordance with those obtained by Rokitansky, Westphal, Meynert, Adler, Lubimoff, Schüle, Mierzejewski, and Herbert Major, with the following exceptions:—

(a.) The so-called colloid bodies are not strictly to be so termed; they are evidently a fusion of several organic compounds, among which lecithin is evidently one.

(b.) Fat and oil globules I never found.

(c.) Amyloid degeneration of the vessels occurs in the luetic form alone, as far as my experience goes.

(d.) There is no correspondence between the preponderance of melancholic symptoms and the development of protagon spheres (Schüle).

(e.) Pigmentation of the cortical nerve cell, where diffuse, cannot be unquestionably interpreted as abnormal since the cells of the substantia nigra and locus ceruleus are always pigmented. But pigment clumps in a nerve cell are decidedly pathological. These do not constantly occur.

(f.) Miliary aneurisms, fusiform and dissecting, must be shown to have a definite relation to the symptoms of the disease before they can be considered of any importance. I am convinced that where the investigator has found them, very frequently he has caused their artificial production by pursuing improper methods. I have not found a single clear appearance of this kind which would stand all tests. Besides, they occur in those who never have been insane as a predisposing cause of apoplexy.

(g.) There is often an apparent disproportion between the gravity of the mental symptoms and the changes in the ganglion cells which appear to be intact. In such cases it is the rank growth of the connective tissue which, with Rindfleisch, we may consider as having rendered the delicate associating and projecting machinery of thought useless.—(*American Journal of Nervous and Mental Disease*, April, 1877.)

Microscopical and Chemical Examination of the Blood in Insanity.—Dr. J. P. Gray, of the State Lunatic Asylum, Utica, New York, in his report of the working of the asylum for 1876, remarks—"We have this year added to our pathological investigation the microscopic and chemical examination of the blood in a number of cases. In all these, four ounces of blood, collected from both sides of the heart and aorta, were reserved for examination. The part of it for microscopic investigation was mixed in a graduated tube, with two parts of glycerine and water, of 1·060 specific gravity. The red and white corpuscles were counted under a one-sixth inch objective in a thin glass cell, made of very thin covering glass, by the aid of an eye-piece micrometer. In all cases, the same graduated tube, the same slide cover, objective, and eye-piece, have been used, and the figures are the average ratio of four examinations in each case. The average number of corpuscles in the diluted blood of a healthy man, in twenty square degrees of the micrometer used, was 162; and the average ratio of the white to red corpuscles, 1 to 450. The amount of iron was determined, in the ashes of 100 grammes of blood, by volumetric analysis, with permanganate of potash. The hæmoglobin contained in the blood was calculated from the amount of iron, and it was found that 100 parts of dry crystallised hæmoglobin = 0·42 parts of iron. Another method adopted, of ascertaining the amount of hæmoglobin, was by examination of the blood with the spectroscope. The amount of iron and hæmoglobin corresponding in the blood of a healthy man is—iron, from 0·0506 per cent. to 0·0633 per cent.; hæmoglobin, from 12·09 per cent. to 15·07 per cent. The amount of hæmoglobin by spectroscopic analysis, from 12·13 per cent. to 15·96 per cent." Dr. Gray then proceeds to record 14 cases, 7 males, and 7 females, in which such examinations were made, and the following Table, which I have drawn up from this record, exhibits the figures obtained:—

Cases	Number of Corpuscles	Ratio of White to Red	Quantity of Iron per cent.	Quantity of Hæmoglobin per cent. by Spectroscope
MALES.				
Case 1	152	1·300	0·0439	10·19
„ 2	154	1·440	0·0463	12·09
„ 3	138	1·426	0·0425	10·17
„ 4	150	1·430	0·0479	11·97
„ 5	148	1·360	0·0417	10·12
„ 6	154	1·460	0·561	12·99
„ 7	—	—	—	—
FEMALES.				
Case 1	140	1·290	0·0856	9·13
„ 2	134	1·258	0·0399	9·76
„ 3	—	—	—	—
„ 4	170	1·430	0·0559	14·42
„ 5	164	1·440	0·0523	12·95
„ 6	138	1·434	0·0369	9·1
„ 7	145	1·340	0·0428	10·46

In all but two of these cases the quantity of red corpuscles was diminished, while in all but one there was a relative increase in the quantity of white corpuscles—in two of the cases (females) this being very remarkable. In the percentage of iron, with two exceptions, some diminution was also noticeable, and the same, with three exceptions, applies to the percentage of hæmoglobin.

White Blood Corpuscles in the brains of those dying insane.—Dr. E. C. Mann, in a paper on “The Pathology and Morbid Histology of Chronic Insanity,” read before the New York Neurological Society, and published in the *Am. Jour. of Nerv. and Ment. Dis.*, April, 1877, remarks—“In my writings on insanity I have called attention to the fact that microscopical examination of blood from insane patients, as compared with the blood from the same number of healthy persons, revealed in the blood of the insane a marked increase in the number of white corpuscles. In making microscopical examinations of brain tissue from chronic insanity, I have noticed repeatedly in different cases lymphoid cells or white blood

corpuscles, and also red corpuscles in small numbers, in the substance of the brain tissue, evidently having emigrated from the blood-vessels. From what I have observed I think that under conditions of inflammatory irritation of the brain an emigration of lymphoid cells takes place on a large scale, the cells or corpuscles by virtue of their vital contractility passing through the walls of the vessels, and penetrating into the brain tissue. . . . We have here two factors which operate in the production of the appearance in the brain of the lymphoid cells, and in some cases of the red corpuscles. First, the undue predominance and accumulation in the blood-vessels of the white corpuscles which obstruct the capillaries, as they move so much more slowly than the red corpuscles, giving as a result an impeded circulation and an increased pressure on the coats of the vessels; and, second, the dilatation of the vessels, so frequently recorded. These two conditions are favourable to the rapid emigration of the white and also of the red corpuscles through the walls of the vessels, and also perhaps the same condition may be produced at times by the obstruction in the capillary vessels becoming great enough to rupture them, permitting in this way the escape of a few blood corpuscles into the brain tissue. Such lymphoid cells would act probably as foreign bodies, and a slow course of inflammation would be likely to be set up to get rid of the intruders. Such an inflammatory process would naturally be of slight intensity and long duration, and these collections of lymphoid cells would tend to become developed into a fibroid structure, resulting in induration of the brain, such as we find in chronic insanity." [Dr. Gray's microscopical examinations of the blood and Dr. Mann's histological investigations appear to support and bear on each other, the appearance in the brain tissue of white blood cells being an expression of their increase, as brought out by Dr. Gray's cases. In regard to the occurrence of white blood corpuscles in the *normal* brain tissue, see last "Report," July, 1877.—*Rep.*]

IV. THERAPEUTICS.

Metallotherapy.—The metaltherapeutic method of treating certain nervous diseases was invented by Dr. Burq more than twenty-five years ago, but met with but little response from the profession till about two years since, when it was made the subject of investigation and experiment by M. Charcot at the Salpêtrière. Dr. Burq's theory is that in certain nervous diseases

plates of metal placed upon the skin have the property of altering general and special sensation, and cutaneous vascular supply. The susceptibility of patients to the metals generally used—gold, silver, copper, iron, zinc—varies; a patient sensitive to one metal will be insensitive to another. When, however, it has been established by experiment to which metal the patient is susceptible, this is an indication, according to Dr. Burq's theory, that this metal is the therapeutical agent to be employed in the cure of the disease. These views of Burq's have been submitted to the test of experiment by a commission appointed by the Société de Biologie, consisting of MM. Charcot, Luys, and Dumontpallier, who have drawn up a report of their investigations, the conclusions of which are in accordance with the results formerly obtained by Burq.

Since then M. Charcot has subjected the four patients who were experimented on by the Commission to the *internal* administration of the metal for which they had evinced a peculiar idiosyncrasy, with, in every case, the result of restoring or improving general and special sensation, increasing muscular power, and bringing back cutaneous circulation. These patients were marked examples of aggravated hysteria, or hystero-epilepsy with hemianæsthesia, and in all the disease had lasted, previous to the metallotherapeutic means being employed, for a lengthened period. In the first case chloride of gold and sodium in $\cdot 3$ -grain doses was given, commencing on June 11th, and on June 22nd sensation was normal everywhere, while she at the same time increased in weight, the catamenia, absent for two years, reappeared, and her appetite became voracious, since then the cure has been maintained. In the second case zinc and gold were administered, commencing on June 30th, and by July 10th general sensation was everywhere re-established, with increase of appetite, disappearance of the former hysterical attacks, and, finally, complete cure. The third case was one of hysteria following epilepsy, also affected with anæsthesia. Gold was administered on July 6th, and at the end of the month sensation was restored—the epileptic seizures still continue. The fourth case was one of hystero-epilepsy, and was sensitive to copper, which was administered in the form of the hydrated binocide—first in $\frac{3}{8}$ -grain and afterwards in $\frac{3}{4}$ -grain doses—in conjunction with Saint Christan water (which contains sulphate of copper in small quantities). Under this treatment she was improving, sensation began to return, and menstruation was regular, when she refused to submit to the treatment any longer, and con-

sequently fell back. Subsequently she was induced to take two glasses of Saint Christan water alone, and, though this contains but 3 milligrammes ($\cdot 045$ grain) of sulphate of copper per litre, she soon recovered sensation and muscular force. Against these successes must be placed the failures. M. Magnan, at the Asile Ste Anne, has been unable to obtain results like those of M. Charcot, and Dr. Westphal, of Berlin, has, it is stated, met with similar failures.—(From the *London Medical Record*, Nov. 15, 1877.)

Coloured Light in the Treatment of the Insane.—Dr. Rorie, of the Dundee Asylum, thus records his experience of Dr. Ponza's coloration treatment:—"The *blue* room has, unquestionably, greater effect in lessening and subduing the excitement of patients than dark seclusion—I mean putting a patient to bed and shutting out all light. In some cases this effect appears to arise from astonishment on the part of the patient at the novel position in which he finds himself; in others apparently from the soothing influence of the light itself. In some cases patients seized with sudden violent paroxysms of excitement have become quite quiet and fit to mix with others in ten minutes' time. . . . The *red* room has not been so much used as the other, and has, with one exception, given negative results. This was a case of a general paralytic, who, instead of being blessed with the usual euthanasia common to such cases, was dull, whining, and depressed. On his removal to the red room (at first merely as a matter of convenience) he was found to be cheerful and happy, and whenever taken from the room entreated to be taken back to it, apparently feeling relief and satisfaction in basking, so to speak, in the red rays. Altogether I have been so satisfied with the result that I have had a blue room fitted up at the female side of the house as well as the male side. Before any definite opinion can be given, however, I think the experiment will have to be tried on a much larger scale—say fitting up the windows of an ordinary refractory ward, containing from 12 to 20 patients, as in the wards of an ophthalmic infirmary."—(*Journ. Psychol. Medicine*, April, 1877.)

Massage.—Massage, or muscle kneading, originally proposed on the Continent—a method of keeping up or restoring the nutrition of wasted muscles—is now being advocated by American physicians and surgeons. Professor Sayre recommends its use in muscular atrophy resulting from diseases of the joints, or deformities, practising it by getting the tops of the fingers well into the inter-muscular septa and then rolling the muscle over the bone or deeper

muscles until it is felt to react under the stimulus thus applied; he disapproves of the method of producing muscular contraction by friction longitudinally on the skin. In weakly people, despite the exposure to the air it involves, this process raises the general temperature $\frac{1}{2}^{\circ}$ to $1\frac{1}{2}^{\circ}$ Fahr., according to Dr. Weir Mitchell. In a case of extreme anæmia, with debility, emaciation, &c., the latter physician obtained a most successful result where every method of treatment previously tried had failed, by combining absolute rest (which alone had also failed) with massage and the induction current. "At first, as always, the extremities became cold under massage, then they grew warm, and at last, when she was well, the massage no longer elevated her temperature. And this is the rule."

Dr. Douglas Graham (*New York Medical Record*, April 28, 1877) reports several cases of muscular trouble from over-exercise of certain sets of muscles, and one or two of incipient writers' cramp, that were cured or materially benefited by massage. In one case, that of a pianist, it was of no benefit as far as applied.

ENGLISH SEASIDE WINTER HEALTH RESORTS.

At the usual monthly meeting of the Meteorological Society, London, held on Wednesday, Feb. 20th, at the Institution of Civil Engineers, Mr. C. Greaves, F.G.S., President, in the chair, Dr. Tripe read a paper on "The Winter Climate of some English Seaside Health Resorts." The places selected were Scilly, Torquay, Penzance, Guernsey, Barnstaple, Ventnor, Llandudno, Ramsgate, and Hastings, and the climatic features of each were compared with those of London. The results of this discussion may be briefly summed up as follows, viz.:—The mean daily winter temperature of these seaside places, and especially of those situated on the coast of Devon and Scilly, is higher than at London; the mean daily maxima and minima are also higher, and especially the latter, so that the daily and monthly ranges of temperature are smaller; the mean humidity is less; the general direction of the wind about the same; but the number of rainy days and the rainfall are greater at the seaside. As regards the wind, therefore, the chief point to be especially noticed is the amount of shelter afforded by high land, as at Ventnor, and especially of protection against the stormy and cold winds which ordinarily prevail at the end of February and in March. The soil, also, should be considered, as heavy rains at gravelly and chalky places are not so objectionable as on clayey ground. The discussion on this paper was adjourned until the next meeting, which was held on March 20th.

PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

SAMUEL GORDON, M.D., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, March 6th, 1878.

DR. GORDON, President of the Society, in the Chair.

DR GRIMSHAW read a paper "On the Distribution of Cholera in Dublin in 1866, and its relation to the Geological formation of the Dublin District." [It will be found at p. 308.]

DR. LALOR thought that the paper neglected a most important factor in the spread of cholera—namely, contagion. He (Dr. Lalor) was a very strong believer in the contagion of cholera, and he believed that experience such as he had had would have led anyone else to the same conclusion. Any theory that neglected contagion in cholera was, he thought, radically faulty. As to the course followed by that disease, his belief was that it followed the courses, not of extinct water-beds, but of rivers, and of those chiefly which were commercial routes. Dr. Lalor referred to the geological constitution of the land on which the Richmond District Lunatic Asylum was situated, and of which he had personal knowledge. He commented on the absence of limestone gravel, and remarked that its absence from the material used in repairing the streets of Dublin was one of the causes of their dirty condition and unwholesomeness. As to cholera taking the course of old river-beds, he objected to any general conclusion on that point being drawn from the experience of one outbreak in 1866. He also objected to any conclusion being founded on a vague use of the term "gravel;" and, above all, he objected to any conclusion as to the propagation of cholera by water-courses without account being taken of whether the water-course was a real river or a sewer, like the Liffey and Dodder.

DR. HENRY KENNEDY reminded the Society that a very elaborate report, dealing with this question, had been written by the late Dr. Graves. He, Dr. Kennedy, had had experience of the first outbreak of cholera in Dublin in 1832; and he remembered that a very severe visitation of the disease occurred at Finglas. Another outbreak of the disease of a most virulent form took place at Dalkey, a number of the very worst cases occurring in houses at the top of a quarry. For his part he would be slow to accept the contagion theory with respect to cholera. He had seen a great deal of the disease which made him opposed to it. Dr. Kennedy also referred to the frequent simultaneous existence of epizootic and epidemic diseases; and thought that that fact should be taken into account in any theory intended to explain the operation of epidemics amongst human beings.

DR. CRANNY.—Can Dr. Grimshaw inform us whether cholera has followed gravel-beds and river-courses in districts of the country outside of Dublin?

DR. DARBY believed that cholera was non-contagious, and cited instances from his own experience in support of this view.

DR. GRIMSHAW reminded the Society that he had not himself advanced any theory. He had been merely discussing that advanced by Pettenkofer.

DR. J. W. MOORE held that there was an analogy between the contagion and etiology of cholera and of typhoid fever. There could be little doubt that when the virus of cholera was discharged into gravel-beds it underwent a process which rendered it fertile and caused it to spread through the neighbourhood. If that were true, the theory of Pettenkofer strongly supported the theory of contagion.

DR. GRIMSHAW said one of the points assumed in his paper was that cholera was contagious. If it were not contagious the theory of Pettenkofer could not hold water in any shape. The only theory of contagion with respect to cholera held anywhere was that the discharge from the patient's bowels was capable of reproduction or development; and that the introduction either of the matter discharged or of the product of it into the bodies of other persons propagated the disease. As to the disease following river-courses, it was admitted that it did so, in so far as a river afforded means of transit or traffic, like a railroad, an ordinary road, or the sea. That was what was generally meant by cholera following a river course. It undoubtedly followed the Great Southern and Western Railway during the last epidemic and also went to Balbriggan. With respect to the geological strata of Dublin, Dr. Lalor, in speaking of gravel, had described what he (Dr. Grimshaw) would call boulder-clay. That was the impervious stratum he had spoken of. The Geological Survey had only mapped it within the last twelve months; and until he (Dr. Grimshaw) asked for the information the map was not

drawn. He had seen the Dublin gravel-beds opened in a great many places, and more particularly in a gigantic way near the King's-bridge by Messrs. Guinness in order to obtain lime for their new buildings; and the gravel-stones so obtained were burned and yielded very good lime. Finglas, in every cholera epidemic, had been a focus of the disease; and it was situated on a gravel-bed. As to Dalkey, not only was the quarry-hole used as a cesspool, but the water was drank by the miners; and that invariably spread the disease. Cholera was very virulent in Kingstown, which was situated on a granite district, although some of the granite was overlaid with boulder-clay. He deemed that the occurrence of isolated cases of cholera did not disprove the theory of contagion. They did not yet know anything about the incubation stage of cholera. Pettenkofer held that it was indefinite, and that cholera might be carried by a person who did not get the disease at all. It was pretty certain now that a person might swallow cholera contagion, carry it about, even, bring it from the East, and not get it himself, and finally plant it in a gravel-bed where it would grow for somebody else. It was his intention at a future day to compare the paths of cholera in the country districts with the geological maps, when the Geological Surveys were sufficiently complete for the purpose. It had been shown by the reports of Cunningham and Lewis in India that those points were of considerable importance. Those gentlemen were still pursuing their inquiries. He believed that Pettenkofer had shown good grounds for his theory, and that it had been very much borne out by their experience in Dublin.

The Society then adjourned.

TREATMENT OF PARALYSIS IN POTT'S DISEASE.

CAUTERISATION of the back is advocated by Dr. Poore, of New York, in the treatment of cases of this class. He gives the following directions for its application, and if used accordingly there is, he states, no pain or subsequent suppuration:—1. The iron should be olive-pointed, and *perfectly smooth*; there must be no thin scales of oxidised metal on it, otherwise it will scratch the skin and make a sore. An iron with a platinum cap spun on has always a smooth surface, and does not oxidise. 2. The iron must be raised to a *white heat*; a lower temperature always gives pain, and makes a sore. 3. In children the parts to be cauterised should be chilled with ice and the skin then wiped *perfectly dry* before applying the iron. 4. The iron must be simply brushed over the skin, so that after the operation there is only a whitish line to be seen. Dr. Poore has frequently cauterised the backs of children without their making any complaint; they have been put back in bed, and immediately returned to their toys as though nothing had been done.—*N. Y. Med. Jour.*, Feb., 1878.

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

SESSION 1877-78.

President—G. F. WALES, M.D.; F.R.C.S., Ed.

Hon. Secretary—WILLIAM WHITLA, M.D.

Wednesday, the 2nd January, 1878.

The President, DR. G. F. WALES, in the Chair.

Adjourned Debate on Alcohol.

DR. WHEELER gave a history of alcohol and its preparations in every known form, quoting from the Sacred Record, and from Herodotus and Homer, to show its effects on body and mind. He condemned the use of alcoholic fluids as beverages, on the grounds that they were not only unnecessary in health, but positively injurious, and from long experience and patient observation endorsed fully the statement, "that he who drinks will drink again, and that moderation (with many) oils the hinges of the gate leading to excess."

He quoted from a very able paper of Dr. Wilks, physician to Guy's Hospital, and also from Dr. Parkes's paper on the use of a spirit ration in the Ashantee Campaign, which proved that alcohol is not a stimulant, nor a trustworthy aid. He also quoted the opinion of Dr. Parkes and other well-known workers, who agree that the use of alcohol in health is in no way beneficial, but on the contrary, in many instances, decidedly injurious, by exhausting the vital powers and inducing disease. He finished by saying it was needless to dilate upon the dangers which surrounded the use of it as a beverage, and believing that moderation so frequently leads to intemperate habits, thought that they, as medical men, should be very careful that in no way they should encourage the drinking habits of the age.

DR. ESLER said:—Mr. President, the limits put to this discussion by the very terms of the proposition makes it so narrow that one is almost forbidden to touch upon the moral, the social, or the therapeutic aspect of the question.

In thinking this subject over, and in listening to the many speakers who have preceded me, I am driven to the conclusion that the issue arising from the first proposition is too narrow, and for this reason—that you take a standard of health as the condition requiring to be benefited. Now *health*—that is, *good health*, cannot be improved upon, and unless alcohol be proved to be food, and a harmless food, even a desirable food—and until it be shown that an increase of the heart's action, that is, of the work done by the heart, is not in any degree hurtful to the animal

economy—I, for one, fail to see that it is *beneficial in health*; besides tens of thousands of healthy men with practical experience, and a vast array of the best names in the medical profession, all testify that alcohol in health is unnecessary.

Recently a correspondent of some eminence in *The Lancet* tried to show that in his case a small quantity of alcohol daily was necessary to maintain good health and digestion, detailing three different occasions on which he had given it up, and on each occasion his tongue furred and his appetite failed. My reading of the case was, that this eminent member of the profession had acquired an artificial taste or habit, by which the peptic glands were stimulated to pour out their secretions, and without which they would not act well. Perhaps a little more perseverance would have overcome the difficulty.

Against this case I do not think it would be difficult to quote a number where the *use*, even the moderate use of alcohol, has produced results exactly similar to that produced in this case from abstinence, and the lesson I would draw from it is one for youth—viz., not to acquire the habit, for habit, any habit, and especially the drinking customs of society, are too strong to be easily thrown off.

The second aspect of the question is put thus, “Is the moderate use of alcohol injurious?” The answer to which need not turn on the definition of the word *moderate*. That its *immoderate* use is injurious no one will question.

Alcohol injurious? Yes. And alcohol in moderation injurious? Yes, and for these reasons:—It fosters an artificial state of appetite, digestion, existence; it is always a dangerous boundary line on which to walk, and *immoderate* and *injurious* are moderation’s nearest neighbours; it is a bad example to the young, and to others who have not full self-control. This, to medical men, is an important element in weighing the question, as none of us dare venture to ask, “Am I my brother’s keeper?” That is our trust, whether we answer the question or no.

It is an expensive habit, and *this* to a great many people is injurious enough. Many a man’s pocket-money spent in beer, and many a woman’s pin-money spent in gin, leaves the fire small on the hearth, the bed-covers thin, cold, and comfortless, and the children’s boots leaking at every point. Remember, too, that its estimated cost to this nation alone is nearly a hundred and fifty million sovereigns in gold.

We have heard a good deal in this discussion about light wines, as if light wines were to bring in a reign of moderation, and it has been affirmed that there is little excess in wine-growing countries. Sir, history and experience have written on the very forefront of wine—DANGEROUS. I am sorry I have not Continental experience, like some of my predecessors on this question, but I have been over a large extent of the wine-growing districts of Victoria, and I can testify to the old

experience that it is not uncommon to err through wine, "to err in vision and stumble in judgment." I have been assured by those who have practical knowledge of the whole question from their residence in grape-growing districts, that the most chronic form of tipping, and one of the most insidious, is that induced by the constant use of wine; and, leaving out of account the gouty diathesis to which it may lead, I am not inclined, with my present information, to subscribe to the doctrine that light wine is the antidote required to stem the ever-widening torrent which has already devastated, and which threatens yet more extensively to destroy the medical, the social, the industrial, and the moral relations of society. I rather subscribe to the old statement, "Wine is a mocker."

In conclusion, sir, if I say that, as a therapeutic agent, I consider alcohol almost invaluable—certainly indispensable—but that in health, especially the health of youth, I do not think it is necessary; that it is only exceptionally beneficial, and generally hurtful, and that it is injurious in a degree proportionate to the quantity taken; that its use, when indulged in as a luxury, should be in extreme moderation, only with food, and not habitually, you have pretty much my opinion on this whole subject.

DR. SPEER said:—Mr. President and Gentlemen, I shall briefly state my views on this question thus:—

1. That in my humble opinion the moderate use of alcohol in health is positively injurious.

2. That in cases where its use is decidedly beneficial, the person using it is not in a state of perfect health, in which instance it is fairly entitled to be regarded as a medicine, not a food.

3. That a comparison physically of total abstainers and moderate drinkers would tell immensely in favour of the former; so would a comparison on the question of healthiness.

4. That it is a fact proved to the satisfaction of every honest mind that total abstainers can endure greater cold and more excessive toil than moderate drinkers; and I submit that this fact is a telling argument against the moderate use of, or necessity for, alcohol in health.

DR. AICKIN, Vice-President, said:—Mr. President, the question of alcoholic stimulants being necessary or unnecessary in health, seems to suggest a preliminary inquiry—namely, what do we understand by the term health? It appears to be that nice balance between waste and repair of tissue, which the use of ordinary food produces in our bodies. When this state of fair balance exists, we might say the health of the individual is "at *par*." In this we include the healthy action of the nerve-power as kept in the state of natural excito-motor action, by the natural stimulus of healthy blood circulating through the cerebro-spinal system; this cerebro-spinal system in its turn acts upon the whole circulatory system, forcing every separate portion of it to perform its office, whether at the heart or at the ultimate capillaries of each organ

or part of the body, giving it that power which removes effete matter, and appropriating from the passing current of blood what is best suited for replenishing the particular tissue through which it is circulating. This takes place I say when the strength (nerve power) is at par. If this is the state of things in a healthy body, produced by ordinary food, as evidenced by millions of the human race who have never used alcoholic stimulants, then I say alcoholic stimulants are not necessary to maintain a healthy person in health. Contrariwise it will be easy to show, by following the preceding arguments, that their influence on health is generally hurtful. Why? because the nerve-power is over-stimulated. When this is done, the immediate effect is to produce—1st, a too rapid action of the heart, sending blood too quickly through the body, preventing the capillaries taking nutriment deliberately and sufficiently. 2ndly, the effete matter is removed too rapidly, congesting the organs that should eliminate this matter, being for the time overtaxed, and not able to act so hurriedly, being over-burdened. 3rdly, this congestive state is further increased, as we know that after excessive stimulation the strength sinks proportionately as far below par as it was stimulated above it. The effect of this lowered state of capillary circulation is a subsidence of nerve-power below health, producing accumulation of effete matter in capillaries, which means poisoning, requiring a fresh impetus by means of stimulation to get rid of the deposited matter, which if permitted to accumulate will ultimately destroy health, and healthy action of part affected, so that we find that continued stimulation requires, through use and abuse, continued but steady increase to produce the same effect, until ultimately the excito-motor power is so overtaxed that it is impossible for it to respond to the stimulus, and so breaks down through sheer fatigue and want of time for natural resuscitation by nutritious food. This is what takes place when the strength is forced “above par.” Now comes the question—when are alcoholic stimulants necessary? Answer, when the strength is “below par.” This is constantly the effect when disease has produced that amount of debility which requires stimulation. It then in part raises the functions of the different organs up to par, producing stimulation of nervous system, then increased action of heart and capillary system, giving it power to remove effete accumulations, and by its preservative influence preventing too rapid decay of tissue, as constantly takes place in disease; it also stimulates digestion, directly and indirectly (by nervous influence), and also, by its narcotic property, acts as a soothing agent to the then irritable nervous condition produced by disease. From the foregoing reasoning I consider alcoholic stimulants necessary when strength is below par, or in debility after disease; unnecessary when strength is at par; that their influence is generally hurtful in health, and positively injurious when strength is pushed above par.

Dr. WHITLA (Hon. Sec.) said:—Mr. President and Gentlemen, it never occurred to me to speak upon this subject, nor should I even now, but for your informing me that it was your intention to call upon me in this meeting. You want an expression of opinion from men who have spent years in the practice of their profession, and the value of the opinion upon a subject like this is certainly in proportion to the donor's experience. Being one of the junior members of the Society, I would consider it wholly unpardonable if I occupied your time at any length. I had not taken alcohol for twenty years, but after hearing and reading much about its beneficial results, I determined to try its effects upon myself. For a period of two years I took small doses of alcohol daily, with more or less regularity, believing now that its use was highly beneficial, and now that it was as lowly pernicious; and it was not till I had repeatedly ceased, and as often resumed it, that I felt at all satisfied with the result. I found that, in addition to the temporary glow of comfort, dilatation of the superficial capillaries, cutaneous insensibility, cerebral activity, &c., one effect was markedly constant, and as this is the most important of the effects produced by small doses of alcohol in health, and as time is short, I purpose to attentively look into it. I refer to *the increase of appetite*. Those who opened the debate, and who seemed favourable to moderate doses of alcohol in health, have given prominence to this action. Dr. Cuming, who so ably laid down that the groove in which the discussion should run was that of health, immediately proceeded to say that alcohol in moderate doses will give an appetite to a man who has none. Now, is a man without an appetite a fit subject to illustrate the physiological effects of alcohol? Here, I believe, lies our most important and vital issue; and without attempting to define "health," I think, sir, in considering these resolutions, if we would keep in our mind's eye a typically healthy man, who is to be just at the time of our experiment in the condition of perfect health of mind as well as of body, much of the difference of opinion on this matter would vanish, and we could hardly conceive any one unable to say that to such a one alcohol is *necessary*. I found that when *in health*, the consumption of food was always increased at that meal with which it was taken; this is generally admitted, but in my case I found that the total amount consumed in twenty-four hours was diminished. This is too often overlooked; we order small doses of alcohol with dinner to a patient whose appetite is lagging, and we find he eats more, but there is often no benefit. What are we doing? Compelling a stomach to do an amount of work in an hour and a half which should be spread over twenty-four. In such cases alcohol is manifestly injurious. I believe that all men with vigorous appetites eat much more than is necessary for the maintenance of the organism, and this surplus acts as a direct stimulant to the nerve, vaso-motor, muscular, and digestive systems, and produces effects identical with small doses of alcohol. During the two years trial

of alcohol I was weighed about every eight weeks, and only once was there any marked increase in weight; and I was much surprised, *and still am*, that this increase followed a period of total abstinence from alcohol, having entirely suspended it from the time of being weighed till next. This is the more remarkable when we consider that the form of alcohol was strong ale.

Well, Mr. President, to sum up my little experimental experience of alcohol, I must say that experience has led me to entirely discard it when in health, which I have done some months ago, and has brought me irresistibly to the conclusions so ably expressed by yourself to the Council, viz.:—

1. That alcoholic stimulants are unnecessary in health.
2. That their influence on health is only exceptionally beneficial.
3. That their influence on health is generally hurtful.
4. In order to lessen the hurtful consequences attending the use of alcoholic stimulants, I believe that, if used, they should be in extreme moderation—only with food, not before dinner, and not habitually.

I cannot sit down without referring to a few statements made by one who so ably spoke at the commencement of this debate. Dr. Cuming no little surprised me when he stated “that in order to daily experience the physiological effects of alcohol, it was not necessary to increase the dose, but if anything to diminish it.” This is not my experience, and if it were true as a rule, I believe there never would have been the necessity for this Society discussing for four nights the sad question of alcohol. To those whose appetites are good, but who wish to be “throned over petty annoyances,” I cannot recommend its use, or call it “an innocent gratification,” knowing by what an awful train of woe this gratification is often followed; and the fact of our being physicians does not release us from the terrible responsibility of being our brothers’ keepers.

The PRESIDENT then read the following resolutions of the Council of the Society:—

“The Society considers,

- “1. That alcoholic stimulants are unnecessary in health.
- “2. That their influence on health is only exceptionally beneficial.
- “3. That their influence on health is generally hurtful.

“4. In order to lessen the hurtful consequences attending the use of alcoholic stimulants, the Society advises that, if used, they should be in extreme moderation, only with food, not before dinner, and not habitually.”

To the first resolution Dr. Dill moved, and Dr. Dempsey seconded, an amendment, “That the Society considers the use of alcoholic stimulants are not *generally* necessary in health.” The amendment was put to the meeting and lost.

The resolution “That the Society considers that alcoholic stimulants are unnecessary in health” being put to the meeting, was carried.

The second and fourth resolutions were withdrawn.

The third, "That their influence on health is generally hurtful," was carried by acclamation.

The PRESIDENT then made the following remarks:—We have now reached the conclusion of the debate on "Alcohol: is its moderate use in health beneficial or injurious?" and, after full and free discussion, we have decided by a majority of votes that it is *unnecessary and generally hurtful*.

I think we owe much to our senior and leading brethren for coming out and giving us so freely of their valuable experience on this important question, which, I am sure, will be read with interest and profit by all to whom the promotion of temperance is a matter of concern, and I consider it highly satisfactory that members generally, and more particularly my junior brethren, have declared themselves so emphatically on what I regard as the correct side in its scientific aspect, but which, beyond doubt, is the safe side.

I would not have it understood that I condemn the *occasional moderate* use of alcohol in any form in health, or say that it necessarily does harm, but I do condemn and fear the daily habit of using it even in strict moderation, because a want is thereby created, on the regular supply of which comfortable feeling largely depends; and when a healthy man finds his happiness in any degree involved by his not swallowing a certain amount of alcohol daily, his independence is already largely compromised, and he is liable to be enslaved by the most seductive, tyrannical, and dangerous of all influences; for it must be remembered that it is out of the habitual, and at first moderate, taking of stimulants that excesses grow, and victims are made. How many these are may be assumed by considering the amount of money spent by the people of these kingdoms annually in the purchase of intoxicants—£147,000,000! Enough in little over five years to pay off the national debt; enough, if spent in supplying food, clothing, domestic requirements, and otherwise beneficially, to develop good trade, the physical, mental, and moral well-being of the people, national greatness, and general prosperity—an average of £23 a year for each family! of nearly £5 for each man, woman, and child! And what do the people get in exchange for all this expenditure? Misery, misfortune, disease, crime!

Your vote this night must have considerable influence in awaking many to a sense of incipient danger. It is a most unselfish vote, for, next to the revenue and the publicans, who benefit so much as we by the intemperance that prevails? It is a vote I look on with great satisfaction, as the first act of the Ulster Medical Society in my year of office, and it is one, gentlemen, which I feel well assured we will never have cause to regret.

Dr. DILL moved, and Dr. Wheeler seconded, a vote of thanks to the President for his conduct in the chair during the debate, which was passed by acclamation, and which brought the meeting to a close.

CLINICAL RECORDS.

Case of Aphonia, successfully treated by Excision of both Tonsils. By
EDWARD C. THOMPSON, M.B.; Surgeon, Tyrone County Infirmary.

A LADY, who lives in the neighbourhood, consulted me in the month of last September, about the health of her little son; she spoke in much distress, evidently fearing that nothing could be done for her child's relief, as all the remedies she had hitherto tried seemed to have done more harm than good. The patient was a young lad of about eleven years old. Up to last May twelve months he had always been strong and healthy; he had then contracted a severe cold, which was attended with laryngeal pain, loss of voice, and considerable fever. From this attack he slowly recovered, but never regained his voice. Latterly he had been rapidly losing flesh, and seemed to suffer considerably from dyspnœa; his appetite was very bad, and he took no interest in any amusement. When seen by me he was in a very emaciated condition; his face was pale and haggard, the shoulders stooped, breathing quick and short; pulse feeble; tongue covered with whitish fur; constant tickling cough, with loud gurgling noise in trachea, but no expectoration. He perspired profusely at night; his sleep was restless and disturbed. There was complete loss of voice, every attempt at articulation ending in a sound as if he were attempting to speak with his mouth under water. There was slight tenderness on pressure along the larynx, and considerable difficulty in swallowing even fluids. At first I was inclined to think the case was one of phthisis with ulceration of the epiglottis and larynx; a careful stethoscopic examination, however, revealed no trace of any pulmonary affection. On opening the mouth and depressing the tongue, the tonsils were seen greatly enlarged and perforated in every direction by numerous small ulcers. An examination with the laryngoscope, which was made with much difficulty, plainly showed considerable swelling, and consequent narrowing of the glottis. The enlarged tonsils seemed to rest on the arytenoid epiglottidean folds, where they had formed regular little cups to receive their pendulous points.

The mother's principal object in consulting me was to ascertain whether or not her son was suffering from phthisis; her astonishment, therefore, was very considerable when I told her that I had a very favorable opinion of her child's case, explaining to her that all the distressing symptoms were due to the irritation, swelling, and consequent narrowing

of the glottis, set up by the pressure of the enormously enlarged tonsils, and that when they were removed the patient would most probably get rapidly well. After some persuasion I was allowed to excise both tonsils; this was rapidly and readily effected by Luér's tonsillitome, without much pain or hæmorrhage. Almost immediately after the operation the child expectorated a large quantity of viscid mucus, and was able, when leaving my study, to speak quite distinctly. The mother was recommended to keep the child as quiet as possible in a well-aired room at temp. of 60° F., and to use constant gargles of tepid water, with hot stupes to the throat; food to consist entirely of liquids.

Progress of Case.—A day or two after the operation I received a very good account of the patient's progress; great difficulty, however, was still experienced in expectorating the copious mucous secretion. Ordered:—

R.—Olei terebinthinæ, ʒi.

Aquæ bullientis, ʒxii.

Inhaletur vapor.

He was also directed to take 20 minims of compound syrup of phosphate of iron three times a day.

In the course of a week the patient was up and out; he had completely regained his voice, and was able to enjoy his existence, which, for fifteen months, had been a burden to him. The anxious haggard look had completely left his face, and in its stead was substituted a bright, happy, smiling countenance.

Remarks.—This is an interesting case in many particulars. It is rare to find the tonsils so enormously enlarged as, by their pressure on the glottis, to cause complete aphonia. Romberg—in enumerating the signs of palsy of the recurrent laryngeal branch of the par vagum, viz., “fits of oppression amounting almost to suffocation, noisy hissing respiration, whispering hoarse voice, dyspnœa aggravated by every inspiratory act”—accurately demonstrates the most prominent symptoms present in the case which I have just detailed. The general phthisical appearance of the patient might have led to a mistake in diagnosis. The tonsils, although greatly enlarged, would hardly have been considered the cause of such severe symptoms had not the laryngoscope clearly revealed the amount of irritation which they were producing by their pressure on the glottis. Luér's tonsillitome is a most perfect instrument; any one who has removed the tonsils by the old plan of the ecraseur must be well pleased at the introduction of such an admirable instrument. The inhalation of turpentine acted almost like a charm; it not only caused expectoration of the mucus when secreted, but very shortly restrained its further secretion. The patient is now, eight months after the operation, quite well and strong.

Case of Inertia of the Uterus treated by Electricity. By MARCUS GIVEN, L.R.C.S.I., L.K.Q.C.P.I.; Medical Officer of Blessington Dispensary District.

As I believe electricity has not often been used in cases of inertia, I hope the following may prove of interest to some of the readers of the *Dublin Journal of Medical Science*:—

Mrs. J., aged twenty-two; anæmic complexion; pregnant for the second time; the first child—now twelve months old—was an eight months' child, but she now believes herself to have arrived at the full time. In the morning and early part of day of February 18th, she suffered from pains in the back, accompanied by slight hæmorrhage; but when I saw her in the evening both had stopped; and on making an examination, per vaginam, found the uterus very high, os dilated to the size of a sixpence, and through which I felt a natural presentation. The pains did not return again until the evening of the 28th, when she said they were very severe, and lasted the entire night, but ceased in the morning (March 1st). When I saw her, about 9 a.m., I found the os dilated to the size of half-a-crown, and the uterus much lower in the pelvis; but as she appeared weak and exhausted I gave her 20 grs. of chloral, which had the effect of producing five hours' refreshing sleep; she then got up and walked about the room at intervals during the evening, but the pains did not return until 11 p.m., and again lasted during the night and ceased in the morning (March 2nd), when I found the os dilated to the size of a five-shilling piece. Being again weak and exhausted, I gave 20 grs. of chloral and 10 m. of liq. opii. sed.; and determined, when I returned in the evening, to try if electricity would not hasten her labour. On arriving, at 6 p.m., I found her sitting up, quite free from pain, and had slept a good deal during the day. I at once got her put to bed, and applied one conductor of the electro-magnetic battery on the sacrum and the other on the fundus uteri. In about five minutes there was distinct and firm contraction of the uterus, accompanied by the usual pains; this continued about five minutes, when I stopped the battery and gave her a hypodermic injection of ergot, hoping that the contractions would now continue, without having again to resort to the battery; but in this I was disappointed, for, although the pains continued, they were so weak that I could not perceive any contractions of the uterus, by placing my hand on the abdomen; so I applied the electricity as before, with the effect of at once producing a firm contraction, during which I ruptured the membranes and allowed the escape of a large quantity of liquor amnii. I now stopped the battery, thinking that the uterus, not being so much distended, would contract without any artificial aid, but I was again disappointed; so I now used the battery with each pain (stopping it between them), and in less than

an hour she gave birth to a small healthy female child; but on applying my hand to the abdomen I distinctly felt another in utero, and on examining, *per vaginam*, found a natural presentation. I now kept up firm pressure on the uterus for half an hour, but as no contraction ensued, I again resorted to electricity, with the same happy result, ruptured the membranes, and in a few minutes she gave birth to a small feeble male child, and in five minutes more a large single placenta came away (with both cords attached near the centre), leaving a firmly contracted uterus; but, remembering its previous relaxed state, I gave another hypodermic of ergot, although I could not say the first had had any effect; however, no hæmorrhage ensued, and up to the present (March 14th) mother and both children are doing well.

TREATMENT OF ASTHMA BY IODIDE OF ETHYL.

At a meeting of the Académie de Médecine on the 29th Jan., M. Sée made a communication on the employment of iodide of potassium and iodide of ethyl in asthma (*Gaz. Méd. de Paris*, Feb. 2). During the paroxysm the patient is given an inhalation of from five to ten drops of the iodide of ethyl. Immediately the dyspnœa disappears, and the respiration becomes easy and regular. In order to prevent return of the spasm, he prescribes, during the intervals, iodide of potassium in doses of from twenty-three to forty-five grains a day. M. Sée has found this plan of treatment most successful in twenty-four cases which he has watched for several years. The iodide of potassium and the iodide of ethyl do not appear to act so well in cases of asthma when given separately. They may also prove very beneficial in all other forms of dyspnœa, whatever the cause. M. Rabuteau (*Gaz. Hebdom.*, 15 Fév.) has communicated to the Société de Biologie (Séance du 9 Fév.) the results of his experiments with the iodide of ethyl, the new remedy employed by M. Sée in the treatment of asthma. He finds that it is an anæsthetic, and that its action is less rapid than that of chloroform, but more prolonged. After inhaling the iodide of ethyl himself, he found iodine in notable quantity in his urine—in fact, it is one of the richest substances in iodine. It is decomposed in the system into iodide of sodium and alcohol, according to the following formula:—

$$\text{C}_2\text{H}_5\text{I} + \text{NaHO} = \text{NaI} + \text{C}_2\text{H}_6\text{O}.$$

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

*Of Eight Large Towns in Ireland, for Four Weeks ending Saturday,
February 23, 1878.*

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES								Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea		
Dublin, -	314,666	747	806	10	30	2	1	33	22	11	33·5	
Belfast, -	182,082	525	477	1	19	4	1	4	35	14	34·3	
Cork, -	91,965	200	247	—	16	—	—	2	4	6	35·0	
Limerick, -	44,209	96	127	—	2	1	—	—	3	5	37·3	
Derry, -	30,884	75	52	—	—	—	—	1	1	—	21·8	
Waterford, -	30,626	84	111	—	22	—	—	9	1	1	47·0	
Galway, -	19,692	49	51	—	—	1	—	—	—	—	33·8	
Sligo, -	17,285	28	36	—	—	—	—	—	3	1	27·0	

Remarks.

Except in Derry the death-rate was very high. In Waterford it was again excessive. In London it was 26·2 per 1,000 of the population annually, in Edinburgh 26·9, and in Glasgow 25·5. Omitting the deaths of persons admitted into public institutions from localities outside the registration district, the rate of mortality in Dublin was 32·4 per 1,000. Within the city itself it was 33·9 per 1,000. Zymotic affections caused 133 deaths—the average number in the corresponding period of the previous ten years being 141. The deaths due to zymotics in the two preceding four-week periods were 127 and 137 respectively. Small-pox, whooping-cough, fever, and diarrhoea were more fatal; while measles (although still destructive to life), scarlatina, and diphtheria showed a diminished mortality. Of the 22 fever-deaths, 6 were due to typhus, 15 to enteric fever, and 1 to continued fever of undetermined type. The epidemic of small-pox is unhappily making way in Dublin, as will be seen from the admissions to Cork-street Hospital in the last four periods of four weeks each—in the period ending December 1st, 1877, the admissions

were 8; in that ending December 29th, 20; in that ending January 26th, 15; and in that ending February 23rd, 61. The 10 deaths registered were widely distributed, chiefly on the south side of the Liffey. One of them occurred in Rathmines. Measles was still very fatal in Cork and in Waterford. In the latter town whooping-cough has also become epidemic. Fever and measles showed a great increase in Belfast. Measles and small-pox each caused 179 deaths in London—the former disease showing a considerable decline, the latter a serious increase, compared with the preceding period, in which the deaths from these diseases were 341 and 146 respectively. Cold weather at the end of January raised the mortality from respiratory affections in Dublin. The deaths were 237 (average of previous ten years=184), including 180 from bronchitis (average=149·8), and 36 from pneumonia (average=16·8).

METEOROLOGY.

*Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W.,
for the Month of February, 1878.*

Mean Height of Barometer,	-	-	-	30·194 inches.
Maximal Height of Barometer (on 21st at 9 p.m.),	-	-	-	30·673 „
Minimal Height of Barometer (on 27th at 3 30 p.m.),	-	-	-	29·502 „
Mean Dry-bulb Temperature,	-	-	-	44·1°
Mean Wet-bulb Temperature,	-	-	-	42·3°
Mean Dew-point Temperature,	-	-	-	40·2°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·250 inch.
Mean Humidity,	-	-	-	86·5 per cent.
Highest Temperature in Shade (on 17th),	-	-	-	59·4°
Lowest Temperature in Shade (on 1st),	-	-	-	27·6°
Lowest Temperature on Grass (Radiation) (on 1st),	-	-	-	22·4°
Mean Amount of Cloud,	-	-	-	79·1 per cent.
Rainfall (on 14 days),	-	-	-	1·576 inches.
General Direction of Wind,	-	-	-	W.

Remarks.

Atmospherical pressure was singularly high during the greater part of February, but the weather was nevertheless very mild and open, contrary to what usually happens with high barometers in winter. The anticyclone which advanced over Western Europe from the Atlantic at the end of January persisted until Saturday, the 9th, when the first of a series of bourrasques approached Ireland from S.W. Until the evening of the 8th the barometer was continuously above 30·50 inches in Dublin, where calms or light variable airs and very dull, chilly, and foggy weather accompanied this high pressure. Sharp frosts occurred on the 1st and 7th. On the 9th rain fell in Dublin for the first time since

January 29th. On the 13th a cyclonic centre passed nearly over or a little to the N. of this city—rain fell heavily (at Kingstown .95 inch, in Dublin .83 inch, and in the Phoenix Park .66 inch), and the wind shifted from E.S.E. to W.S.W. with a fall of temperature. Heavy sleet, snow, and hail fell subsequently. Very mild open weather soon set in, and continued to the end of the month. On the 17th a clear sky and warm sun sent the thermometer up to 59.4° in the shade. A second anticyclone formed on the 18th, and the barometer rose to 30.673 inches by the evening of the 21st. Pressure gave way considerably during the last few days, so that on the 27th the reading of 29.502 inches was recorded. At 3 15 p.m. of the 15th a severe hail-shower passed across Dublin with “festooned” clouds. The mean temperature was the same as that of February, 1877, but about 2° above that of the corresponding month in the previous ten years.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

WRITER'S CRAMP.

M. GALLARD, in his recent volume, *Clinique Médicale de la Pitié*, has come to the conclusion that writer's cramp is certainly a professional disease, but a clinical study of it decidedly shows that it is far from being special to persons who write much. Absolutely similar disturbances are seen in persons who follow other avocations, such as engravers, artificial flower-makers, pianists, violinists, telegraphists who use the house-telegraph, &c. But in all these persons the disturbances observed occur in the hands and fingers. Absolutely similar—nay, identical disturbances are observed in persons following professions which exercise other muscles than those of the forearm or hand; and then these disturbances occur in the muscles which are necessarily contracted by the habitual exercise of the avocation, whether these muscles be those of the arm, shoulder, leg, neck, face, or even the trunk. The analysis of these various facts leads the writer to the first conclusion, that the disease in question is not peculiar to writers; the analysis of the symptoms leads him to another conclusion, that it is not a cramp; whence he feels the necessity of substituting for the incorrect denomination writer's cramp, the far more suitable name proposed by Dr. Duchenne of Boulogne—viz., functional impotence. In reference to establishing the nature of this morbid condition, M. Gallard, after having proved that it evades any anatomical localisation whatever, is led to admit that it is a simple functional disturbance and nothing else. He is particularly struck by finding, with regard to its etiology, that fatigue is

far from being an essential cause of it; for this disease does not occur in those who work in a certain way, when the intelligence, otherwise occupied, does not exercise a sufficiently attentive supervision over the muscular movements. This is because there is at that time a veritable discordance between the cerebral acts and the movements, which are performed in a thoroughly automatic manner, that the muscles become fatigued, and finish by performing disordinate movements, that they may be considered as being in an ataxic condition. It is very singular to see a trouble so essentially nervous constitute a morbid functional state which is almost incurable; and M. Gallard in vain seeks the reason of this peculiarity in certain diathetic influences, which he most carefully studies, and which he endeavours to make the basis of a rational treatment. But he is obliged to acknowledge the small efficacy of all the medical means he has successively employed, including electricity; and, tired of the struggle, is reduced to advise prothetic apparatus, in which he does not seem to have much more confidence, although he describes them with great minuteness.—*Lond. Med. Record*, Jan. 15, 1878.

CAFFEINE IN CEPHALALGIA.

CITRATE of caffeine in doses of two grains every half hour until the pain ceases is strongly advocated as an effectual remedy. Often one or two doses are quite sufficient. The only contra-indication is sleeplessness, which sometimes results if it is taken in the evening. It is preferable to guarana as being hardly ever rejected by the stomach. In hay-fever, spinal irritation, and general neuralgia, it would seem worthy of a trial.—*Montpellier Médical*. S. W.

INJECTION OF ACETIC ACID IN CANCEROUS TISSUE.

A MAN, sixty-two years of age, was attacked by a carcinoma of the left horizontal part of the inferior maxilla. Some years before an indurated kernel had been excised from his lower lip. Soon after the operation he complained of lancinating pains in the tongue. The cause was found to be a cancerous abscess, developed in the left half of this organ, near the epiglottis. This new formation was likewise extirpated; to accomplish it, it was necessary to tie, beforehand, the lingual artery and to divide the maxilla. After seven months two other tumours showed themselves—one, small and indurated, on the lower border of the left maxilla; the other, larger and softer, more towards the front of the neck; the last shrank under the influence of applications of ice; the first remained stationary. A year after the extirpation of the tongue there appeared, in the neighbourhood of the right submaxillary gland, another tumour of rapid growth, which resisted the ice treatment. This tumour was likewise a cancer, a microscopic examination being made of a piece extracted by the small harpoon. Into this Dr. Geiss injected, daily, for

a week, acetic acid by means of a Pravaz syringe. The first injection was of the strength of 2 in 9, afterwards it was raised to 1 in 3. Each time the injection was made by a single opening in the skin, but the syringe was turned in several directions, so that the contents of the syringe might traverse a large extent of the tumour. A sharp inflammation followed. On the tenth day a deep incision was made by a bistoury, and a drainage tube introduced, giving vent to a very fœtid, sanious fluid. At the end of seventeen days the suppuration ceased, and there remained only a kernel about the size of a walnut. Soon afterwards four injections were made in the small tumour, on the border of the maxilla, and in a new one which had become developed in the cheek. A fresh tumour took its rise behind the left ear; it became the size of a hen's egg, very hard, and the microscopic examination showed a fibro-cellular stroma, as in the first. Injections were employed, and the author injected every day with two or three syringes, so that in eleven days twenty-five syringefuls of acetic acid solution were used. The first set of injections had provoked no pain—these, on the contrary, were very painful, and were followed by an abundant suppuration, which lasted three weeks, and the tumour completely disappeared. Like success was obtained in the case of a lady who had a cancerous tumour of the breast, the size of a hen's egg, and who would not submit to have it excised. For ten days injections were made; on the eleventh day it was incised. The suppuration ceased at the end of fifteen days, and in four weeks' time there was only a kernel of the size of a nut. The author believes that these injections of concentrated acetic acid will at least hinder the rapid development of many forms of malignant disease, and, in some cases, effect a radical cure.—*Deutsche Zeitschrift für Chirurgie und Revue Médicale.* S. W.

IODIC PURPURA.

IN that excellent new French periodical, the *Revue Mensuelle*, for September, M. Fournier describes a rash consisting in the production of small spots, presenting the characters of purpura which followed the administration of the iodide of potassium. M. Fournier publishes the notes of three apparently conclusive cases, in which this eruption was connected with the medicinal use of the iodide, disappearing when it was ceased. *More Gallico* he sums up as follows:—1. The internal use of the iodide of potassium causes, in some cases, a petechial eruption deserving the name of iodic purpura, or petechial iodism. 2. The purpura is seated nearly constantly upon the legs and on their anterior aspect. It is always discreet, and composed of small sanguineous miliary spots, not causing any local or general disorder, remaining, for that very reason, latent, and disappearing spontaneously in a space of time varying from two to three weeks. 3. It is a rare phenomenon, only

produced in certain patients peculiarly predisposed by natural idiosyncrasy, and particularly liable, in this same class of invalids, to relapses. [M. Fournier is in error if he believes that he is the first to describe this peculiar medicinal rash. His distinguished master, Ricord, observed it, as have also Bumstead, the well-known American syphilographer; Virchow, and Ringer. References to the cases described by these authorities will be found in Dr. Walter Smith's "Reports on Materia Medica and Therapeutics" in this Journal for Aug., 1871; Feb., 1872; and Feb., 1873. In Ringer's case iodide of potassium or iodide of ammonium produced purpura, but iodide of sodium did not.—ED., *Periscope*.]

NEW FORM OF SPHYGMOGRAPH.

In the October number of the *Journal of Anatomy and Physiology* for last year, Dr. W. J. Fleming, of Glasgow, describes a form of transmission sphygmograph which he has devised. A curved brass plate passes down over the front of the wrist into the hollow of the hand. This is held in position by three lead bands. The wrist being placed horizontally on a cushion, a Marey's *tambour* may be brought vertically over the radial artery, and lowered until a rod, perpendicular to its surface, comes to press with its terminal button to any desired degree on the vessel. By means of a series of split weights any given pressure may be applied. The ease and accuracy with which the instrument can be adjusted, the absence of springs, and the non-occurrence of extraneous pressure on the vessels, are among the advantages claimed by the author for this instrument.

R. J. H.

AMPUTATION AT THE HIP-JOINT.

THE latest statistics, we believe, that have appeared of amputation at the hip-joint are by Dr. August LÜLING, and are given in the *Deutsche Zeitschrift für Chirurgie*, Vol. VIII., page 327 (June 15, 1877). They are as follows:—

Traumatic,	.	.	.	Mortality, 85 per cent.
Gunshot traumatic,	.	.	„	88 „
Pathological,	.	.	„	42 „
Re-amputation,	.	.	„	40 „
After previous resection,	.	.	„	50 „
General mortality in 486 cases,	.	.	„	70 „

Of 239 fatal cases, $5\frac{1}{2}$ per cent. died during the operation; $12\frac{1}{2}$ per cent. during the first hour; 26 per cent. in the course of 5 hours—5 of these from chloroform; 46 per cent. in 24 hours. Eighteen cases died from pyæmia, 70 per cent. in 5 days. Eight times death followed from secondary hæmorrhage. [The above figures are taken from a report by Dr. Erskine Mason, of New York, of his third successful case of this kind, in the *N. Y. Med. Jour.* of February, 1878.]

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PART I.

ORIGINAL COMMUNICATIONS.

ART. XII.—*On the Anatomy and Physiology of the White Tracts of the Spinal Cord.*^a By J. M. PURSER, M.D.; F.K.Q.C.P.

I.—ANATOMY.

THE attempt to unravel the course taken by the white fibres of the spinal cord has been made from various sides. The anatomical investigation of the fully-formed organ, physiological experiment, pathological observation, and the study of the development of the medulla, have all been employed. It is my intention to endeavour to give a (necessarily brief and imperfect) sketch of the results which have been attained by each of these methods.

The amount of knowledge which has been gained by direct anatomical investigation of the fully-formed spinal cord is, as regards the course of the conductors, extremely small. The compact and beautifully systematic accounts of the structure of the cord which were given by Van der Kolk and earlier writers have been shown by recent research to rest on imperfect observation directed by preconceived views of that structure which physiological experiment seemed to demand. The influence of these preconceptions has been forcibly pointed out by Vulpian,^b whose position as

^a A Discourse delivered before the King and Queen's College of Physicians February 18, 1878.

^b Physiologie du Système Nerveux. Leçon XV.

anatomist, physiologist, and pathologist enables him fully to appreciate their evil influence. Sensation is a cerebral function; hence it appeared necessary that fibres from the posterior roots should pass direct to the brain; these fibres were immediately seen in the posterior columns. Reflex action required that a connexion between the anterior and posterior roots should exist; forthwith some of the fibres of the latter were seen passing forwards and joining the processes of the cells in the anterior horn, or even a direct communication was demonstrated (Dean) between the fibres of the anterior and posterior roots. Next, physiological experiment seemed to show that some of the sensitive conductors passed downwards for some distance in the cord before passing up to the brain; immediately everyone saw the descending portions of the posterior roots. Finally, experiment seemed to prove that a decussation of sensitive conductors took place in the cord, when straightway this decussation, hitherto unsuspected, was shown to admit of the easiest anatomical demonstration. "And do you ask me," Vulpian continues, "what would happen if to-morrow it should be proved by conclusive experiments that the transmission of sensitive impressions is not crossed, but direct? It is not difficult to foretell what would happen. The decussation of the sensitive fibres would disappear, and men would wonder how they had ever believed in its existence."

Accordingly, we find modern histologists, notwithstanding the greatly improved instruments and methods with which they work, expressing themselves with very great caution, and almost without exception admitting that the task of tracing the conductors through the cord is beyond the powers of anatomical art as it at present exists. Some valuable facts have, however, been ascertained, which would seem to rest on a solid basis. It is, I think, established that all the fibres of the nerve-roots, whether anterior or posterior, pass, sooner or later, into the grey substance. The anterior roots, passing in several bundles, obliquely, through the antero-lateral columns, plunge at once into the anterior horn. Here the bundles break up, and the fibres are seen to run chiefly in the neighbourhood of the groups of large, branching cells which form the most conspicuous feature of this part of the grey matter. These cells were shown by Deiters (whose observations have been confirmed by all subsequent anatomists) to have two kinds of processes. The greater number are freely branched, and, by their ramifications and interlacement, constitute a large part of the fibres which lie in the grey substance,

while from each cell comes off one^a process which, differing in structure from the others, runs an unbranching course, and, at some distance from the cell, becomes clothed with white substance, and, in fact, is converted into a fully-formed nerve-fibre. Deiters, while admitting the possibility that axis-cylinder processes may pass into the white columns and ascend to the brain, has satisfied himself that in some cases they certainly pass into the anterior nerve-roots.^b It seems to me highly improbable that a structure so very definite and characteristic as the axis-cylinder process should have an inconstant course and destination; and I think, since it has been proved that in some cases the process passes into the nerve-roots, while there is no certain evidence to show that it ever passes in any other direction, we may conclude—at all events, provisionally—that its passage is always into the motor nerve, and, in fact, that the fibres of the latter originate in the axis-cylinder processes of the large branching cells of the anterior horn. That this junction of nerve-fibre with nerve-cell is not often seen is due to the long and tortuous course of the nerve-fibres in the cord, so that a single fibre scarcely ever lies for any great length in the plane of a single section. Besides, in ordinary sections the different processes of the cells are not always readily to be distinguished; and in torn preparations it is not possible to isolate a fibre in its entire length, from its junction with a ganglionic cell to its exit from the cord as a motor nerve-root. Gerlach^c has, however, in his isolations, always seen the axis-cylinder process point forwards in the direction of the roots. Boll,^d too, has found this to be invariably the case both in sections and torn preparations; while in one peculiarly fortunate section he saw an axis-cylinder process pass from a ganglionic cell into the bundles of the anterior root, in which it could be followed up to close under the surface of the cord. The passage of the motor fibres into the branched cells of the anterior horn must hence be looked on as one of the best-established facts in the anatomy of the cord. Whether all the motor nerve-fibres end in these cells, or whether some merely pass through the grey matter and ascend to the brain in the white columns, is uncertain, although the former view is by far the more probable.

^a Schiefferdecker has demonstrated cells in the anterior horn of the spinal cord with two axis-cylinder processes, both passing into the anterior roots. *Arch. f. Mik. Anat.*, X., 471.

^b *Untersuchungen über Gehirn und Rückenmark*, 133.

^c *Stricker's Handbuch der Gewebelehre*.

^d *Die Histologie und Histiogenese der nervösen Centralorgane*, 64.

As regards the posterior roots, our knowledge is much less satisfactory. Penetrating the cord in compact masses, they break up into numerous smaller bundles, which branch and anastomose freely. The bundles form two groups—one, which passes at once into the posterior horn; the other, which enters the outer part of the posterior white columns. In each case, before passing forwards, the fibres run for some distance in the long axis of the cord either upwards or downwards. It would seem, however, almost certain that sooner or later all the fibres of the posterior roots enter the grey substance. But here the real difficulty begins. The cells in the posterior horns differ remarkably in size and form; some are as large as the cells in the anterior horns, but the greater number are very small, and they all agree in being extremely delicate, fragile, and difficult to manipulate. Hence there is by no means the same uniformity of opinion about them that prevails concerning the so-called motor cells. Deiters, who attributes to them an arrangement of processes similar to that existing in the cells of the anterior horn, supposes that the posterior roots terminate in the axis cylinder processes of the posterior cells just as the anterior roots do in those of the anterior cells. Gerlach, however, takes a somewhat different view of the matter. He supposes that the cells both of the anterior and posterior horns send their unbranching processes forwards to the anterior roots, while their branching processes form a dense network, from which come off the posterior roots, whose fibres are formed by the coalescence of a certain number of the exceedingly minute filaments by which the terminal network of the protoplasm processes is constituted. This view has some probability given to it by the existence of fine dividing nerve-fibres throughout the grey substance, but particularly numerous in the posterior horns, and by the want of proportion between the number of cells in this part of the grey matter and the number of fibres in the posterior roots. In the anterior part of the cord such a proportion exists at each region, while in the posterior part the fibres are far too numerous to admit of each one of them terminating in a cell, as in the case of the anterior roots. Still the evidence for Gerlach's view is very slender, and it must be admitted that it is, so far, no better than an ingenious hypothesis.

Since the nerve-roots do not directly pass into the white columns of the cord, these must arise from the grey matter. According to Gerlach's theory, they, like the posterior roots, communicate with the network formed by the protoplasma processes of the cells,

whose finer branches coalesce to form their fibres. At several points fibres can be seen leaving the grey matter, and passing into the white columns; thus the fibres which decussate in the anterior commissure pass from the anterior horn of one to the anterior column of the opposite side, and from the lateral and posterior part of the grey matter fibres can be seen leaving this and passing upwards (and downwards?) in the white columns. That some of these fibres run to the brain, while others pass merely from one part to another of the cord, is undoubted, but anatomical investigation cannot tell which course each fibre or bundle of fibres takes. The distinction between the different tracts of white fibres, which we shall see made evident by other methods, can scarcely be arrived at by mere anatomical investigation. The separation between the posterior columns and the lateral is everywhere clear, since the apex of the posterior horn of grey matter with the posterior roots makes a continuous line of division, but the limits of the anterior from the lateral columns is purely arbitrary, and exists only where the anterior roots pass in—the external bundle of each root being taken as the boundary, which, between successive roots, is continued by an imaginary line. Sub-divisions of the antero-lateral columns are not evident; but in the cervical portion of the posterior columns, that part next the posterior median fissure is marked off from remainder by a tract of connective tissue and vessels, and is known as the *fasciculus gracilis*, or Goll's column. There is, however, a difference in the white fibres in different parts of the cord, which we shall see subsequently to be of great importance—namely, a difference in their diameter. This was insisted on by Deiters,^a and confirmed by all subsequent writers. The largest fibres are found in the anterior columns, and at the periphery of the lateral columns, while fine fibres lie in the inner part of the lateral columns, adjacent to the grey substance, and in the posterior columns, more particularly in their inner part, the *fasciculi graciles*, which are distinguished by containing only the finest fibres, the remainder of the white columns containing coarse and fine fibres mixed.

To sum up, then, the results of anatomical investigation, we find:—1. That the nerve-roots all enter the grey matter. 2. That the anterior roots certainly, the posterior probably, there enter into connexion with ganglionic cells. 3. That the fibres of the white columns of the cord arise from the grey matter—most

^a Loc. cit., 127.

probably from nerve-cells. 4. That they pass, some directly, others having undergone decussation partly to higher regions of the cord, where they again enter the grey matter, partly to the brain, including in that term the medulla oblongata and higher parts of the central mass.

But while by mere inspection of the fully-formed medulla the white matter seems to constitute but one whole, it is found when the development of this organ is studied that the cortex is highly complex, consisting of many distinctly separated tracts which can be clearly distinguished by their origin, course, and destination, and whose functions, doubtless, differ correspondingly. It is to Flechsig that we owe most of our knowledge on this subject. During some years past he has published memoirs on the development of different parts of the nervous centres, and recently he has collected all his observations into a volume,^a the appearance of which may be truly said to form an epoch in the history of the anatomy of the nervous system.

It is familiar to every anatomist that the appearance of the nervous centres in young animals, whether embryos or newly born, differs very markedly from that in more mature individuals; that while in the latter it is white, opaque, glistening, in the former it has a gelatinous, semi-transparent, reddish-grey aspect, which is due, not as was at first supposed to a greater vascularity, but to the absence of an important constituent of the fully-formed nerve-fibres—namely, the medullary substance, or white substance of Schwann. Flechsig, who has examined the bodies of sixty-five fœtuses and young children, finds that the development of the nerve-fibres does not occur simultaneously in all parts, but that it follows a fixed and definite order; that certain tracts of fibres assume their white colour weeks, and even months, earlier than others lying in their neighbourhood; that while some parts become white in the middle of fœtal life, others remain grey for a considerable time after birth.

Before describing the several tracts into which the white columns of the cord may be divided by following their development, it may be well to state very briefly what is known as to the histogenesis of the fibres of the central nervous system. The mode of origin of the primary axis cylinders is surrounded by much obscurity. Some writers maintain that they are all formed as outgrowths or processes of developing ganglionic cells. This would appear to be the view taken by Flechsig,^b at least for the fibres of the pyramids of the

^a Die Leitungsbahnen im Gehirn und Rückenmark u. s. w. Leipzig, 1876.

^b Loc. cit., 170.

medulla oblongata. In a foetus 25 cms. long he finds the pyramids composed of naked fibres of scarcely measurable thickness, separated from one another by a pale, finely-granular, albuminous material. The cellular elements are few, and appear to belong altogether to the connective tissue. At an earlier stage (12 cms. long), before the first formation of the pyramids, he cannot find in the medulla oblongata any tissue which could serve as a sufficient matrix for the fibres which are subsequently to appear. Hence, he concludes that they probably do not arise *in loco*, but grow downwards from the brain. He admits, however, that his material was not favourable for the study of the very earliest stage of development of the fibres. Boll,^a investigating the corpus callosum of the developing chick, and Eichhorst,^b studying the spinal cord of the human foetus, agree in deriving the earliest fibres from cells which at first round (Eichhorst) subsequently become spindle-shaped and send off a long process from each pole. Boll was unable, owing to the great rapidity with which the stages of development are gone through in the chick, to determine whether each fibre is the product of a single cell, or whether several cells enter into its formation by coalescence of their processes. Eichhorst, however, has observed this coalescence, and figures fibres with several spindle-shaped, nucleated swellings placed at intervals along their course. The fate of the nuclei, as described by Eichhorst, is very remarkable. These seem to be extruded laterally from the fibre, so that instead of, as at first, interrupting the course of the latter, they now come to be placed at one side of it. The fibres now lie parallel to one another, separated by a small quantity of interfibrillar substance, which is at first homogeneous, but subsequently contains fatty granules. Coincidentally with this granular change a remarkable phenomenon is observed. The part becomes infested by a large number of cells, which present amœboid movements and resemble in most particulars the white corpuscles of the blood, and which are, beyond question, wandering cells. Most of these cells contain granules similar to those between the fibres. The explanations given of this immigration of cells is various. Boll and Eichhorst suppose that it is by their means that the fat necessary for the formation of the medullary sheath is carried to the nerve-fibres. Flechsig, on the other hand, thinks that the fat is formed *in loco* from the albuminous interfibrillar substance, and that it is taken up by

^a Loc. cit., 118.

^b Virchow's Archiv, LXIV., 442.

wandering cells, which possibly effect in it some transforming action, and which, being gorged with foreign particles, lose, to a certain extent, their mobility, and are hence for a time fixed in the tissue. Jastrowitz, taking the exactly opposite view to that held by Boll and Eichhorst, considers the granule-cells not as carriers of fat to the tissue, but as being of use to carry off the excess of fat which is formed between the fibres. This view is, however, rendered improbable by the fact pointed out by Flechsig, that after the disappearance of the granules, the whole number of cells in the part has undergone no diminution. One of the chief points of interest about these cells is that their recognition as a normal element in the nerve-centres at a certain stage of development throws a new light on the disease which was described some years ago by Virchow as congenital encephalitis. Some of the examples of this condition, which is characterised by the presence of large numbers of granule-cells in the brain, were probably merely cases in which this organ was in a particular stage of development when examined. If, however, these granule-cells occur in the grey substance—where, normally, they are never found—or in irregular spots or streaks, or in parts in which at that period of development the formation of myeline should be completed, they must be considered abnormal. In the spinal cord the cells in question appear in the fourth month in the posterior columns; shortly after in the anterior columns; in the anterior part of the lateral columns in the sixth; while in the posterior part of the lateral columns they are found abundantly up to the end of the tenth month. The time during which they exist normally in any one tract is not very long. The granules then leaving the cells (which perish, according to Boll; or, according to others, remain to assist in forming the connective tissues) coalesce, and form a shining sheath around each axis cylinder, and the development of the fibre is complete.

In the white substance of the medulla spinalis there may be distinguished, by their successive development, the following separate tracts, which are enumerated in the order of their appearance:—

1. Fundamental tract of anterior columns.
2. Fasciculus cuneatus (Burdach).
3. Anterior mixed tract of lateral columns.
4. Lateral bordering layer of grey substance.
5. Goll's column.
6. Direct cerebellar tract.
7. Pyramidal tracts.

The periods at which these different tracts are formed are separated by long intervals. Thus the fundamental tract of the anterior columns is formed about the fourth week, while the pyramidal tracts are not present until the

latter end of the fifth month. Although the evidence is somewhat defective, yet still, as far as it goes, it shows that the formation of the medullary sheath of each set of fibres occurs at a certain period (four to four and a half months) after the first appearance of the tract as a bundle of naked axis cylinders. Thus the fundamental tract of the anterior columns, which is formed in the fourth week, becomes white towards the end of the fifth month, while the pyramids which are formed towards the end of the fifth month are still grey at the time of birth. From this it follows that the formation of the medullary sheath does not depend on local influences, but that its occurrence depends on the age of the fibres. The knowledge of this fact gives us a means of tracing the various tracts of fibres in the nervous centres which we did not before possess. There is also some reason to believe that, by following the development of the different tracts of fibres, we can gain important information as to the order of development of the centres of grey matter with which they are respectively in connexion.

I shall now state shortly some of the more important facts which have been made out as to the attachments and mode of development of each of these tracts:—

I. *The pyramids* of the medulla oblongata and their continuation in the cord, present, perhaps, more interest than any of the other columns, and the late period of their development gives better opportunities for study than in the case of the tracts of earlier formation.

As has been said, there is strong reason to suppose that the pyramids and their continuations in the cord are not formed *in loco*, but grow downwards from the brain, as prolongation either from the ganglionic cells or from spindle-shaped cells, such as were described by Boll and Eichhorst. At the commencement of the fifth month, not only is there no trace of pyramids, but there is no tissue in the medulla oblongata which could serve as the matrix for their production; while in the middle of the same month, not only are they present, but are already so highly developed as to form one-ninth of the entire section of the medulla. This rapid appearance speaks strongly in favour of their growth from above. Further, they are persistently absent in cases of that malformation known as *acranus*, where the brain is wanting. And although, from the deficiency of material for observation, it is as yet only a probability that at a certain stage the pyramids, as naked axis cylinders, are present in the medulla, and the pyramidal tracts absent from

the cord, it is certain that they become clothed with white matter from above downwards. There is another very remarkable circumstance which is readily explicable on the supposition that these tracts grow downwards, but quite unaccountable on the hypothesis of their formation *in loco*—that is, the variability in their decussation. Before speaking of this, however, I may notice the probable cause of a decussation of any kind. This, although not necessarily involving a downward growth of the pyramids, is yet readily explained by such a theory. At the point where the decussation takes place the anterior fissure of the medulla suddenly narrows, and the whole nervous axis bends on itself, forming an angle, so that the two pyramids, growing downwards and converging, pass, as it were, into the bottom of a pit, and the direction of least resistance to the further growth of each is evidently that which it takes—namely, to the opposite side and backwards, and we accordingly find, as a rule, that the greater number of the fibres of the anterior pyramid of each side is continued in the cord in the posterior part of the lateral column of the opposite side. The fibres are directly continuous across this decussation, and are not interrupted by interposed nerve-cells, as was supposed by Deiters. As a rule, however, all the fibres of the pyramids do not undergo this decussation; those which lie on the outer side of each pyramid pass, as it were, along the edge of the pit, and not meeting with the resistance experienced by the innermost fibres, continue their course in the cord along the inner part of the anterior columns of the same side—hence we have in the cord corresponding to each pyramid the anterior pyramidal tract (direct), the lateral pyramidal tract (crossed). As already said, the decussation is extremely variable. As a rule, in the cord four pyramidal tracts are found, but in some cases the decussation is complete, and the anterior tracts disappear; or we may find, as another extreme, 90 per cent. of the pyramids remaining uncrossed, and then the lateral tracts are reduced to a minimum, their deficiency being marked by a groove or depression running along the posterior lateral aspect of the cord, while the anterior columns are inordinately developed. Between these extremes all degrees occur, and a want of symmetry is not unfrequent, the decussation being much greater in one pyramid than in the other, or one decussating partially, and the other completely. But whatever may be the variability, this law is constant (supposing the pyramids to be of equal size, which is generally, although not always, the case); the sum of the right

anterior and the left lateral tract is equal to the sum of the left anterior and right lateral tract. This law which follows naturally, if we suppose such a growth of the pyramidal fibres downwards as was described, could not be accounted for if we supposed the fibres to arise *in loco*. As the pyramidal tracts descend in the cord, they diminish in size. This diminution does not occur equably, but takes place more rapidly in the cervical enlargement, where the grey matter is abundant and the nerve-roots numerous, than in the dorsal portion, where these are more scanty.

The lateral pyramidal tract always lies at the posterior part of the lateral column, never extending further forwards than a line drawn directly outwards from the tractus intermedio lateralis. It can always be traced downwards as far as the origin of the second or third sacral nerve. Above and below it reaches the surface, but at the cervical enlargement it is separated from the surface by the direct cerebellar tract. It lies against the posterior horn posteriorly, from which it is separated internally by fibres belonging to the posterior roots. By a peculiar modification of the method of gold-staining, Flechsig has seen the fibres of this tract bend inwards, so as to enter the grey matter, and pass as scattered fibres towards the anterior commissure, where they cease to be visible. That they pass into the anterior commissure is improbable, because the fibres in it are of the coarse variety, while those in the lateral tract are mostly fine. That they are directly continuous with the anterior roots is still more unlikely, not only in consequence of the great coarseness of the fibres in these, but because the fibres of the anterior roots are fully developed at a period when the pyramids have not yet got their white substance, and because, as will appear subsequently, when the lateral tracts have undergone secondary degeneration, the nerve-roots remain intact.

As regards the anterior pyramidal tracts, they pass downwards along the inner aspect of the anterior column, and we have no evidence that they decussate in the spinal cord. Their fibres can be seen to bend into the grey matter of the anterior horns, but there is the same improbability as in the case of the lateral tracts that they pass into the anterior commissure or that they communicate directly with the anterior roots. The distance down the cord to which the anterior pyramidal tracts can be followed varies with the amount of each pyramid which pursues a direct course. If this, at the level of the sixth cervical nerve, amounts to 20 per cent. of

the whole pyramidal fibres, the anterior tracts extend to the upper part of the lumbar region of the cord; if, as is more usual, it amounts to only 6 or 8 per cent., they cannot be followed beyond the middle of the dorsal region.

II. *The direct lateral cerebellar tract* forms another well-defined system of fibres. It lies on each side at the outer and back part of the lateral column. It consists of very coarse fibres, which gain their white substance earlier than the pyramidal tracts but later than the remainder of the lateral columns. It increases in size from below upwards, the increase being most rapid in the upper lumbar and lower dorsal regions. Superiorly it does not undergo decussation but passes in the restiform body to the cerebellum of its own side. In the cord its connexions are with the posterior vesicular columns. These remarkable tracts of nerve-cells, described by Mr. L. Clarke, and generally called after him Clarke's columns, lie on each side, posterior and external to the central canal, at the posterior part of the central grey matter. From their anterior and outer aspect bundles of fibres come off, which traverse the grey substance and the inner part of the lateral columns and bend upwards in the cerebellar tracts. These horizontal bundles, which have been observed by many writers, have only quite recently^a been actually traced to the cells of Clarke's columns, although there was before the strongest reason to believe that such a connexion existed. Clarke's columns being confined to the dorsal and upper lumbar regions, the lateral cerebellar tracts do not extend to the lower part of the cord.

What remains of the lateral column after the removal of the two tracts already described can be divided into two portions.

III. *The lateral bordering layer of the grey substance.*—This, which consists of very fine fibres, lies along the concave outer edge of the grey matter. In the lower dorsal region it is traversed by fibres of the cerebellar tract, and in the lower lumbar region it ceases to be distinguishable. Its fibres below bend into the grey matter and pass at first inwards and forwards, but before reaching the anterior commissure bend outwards, and are very liable to be confounded with the fibres of the lateral pyramidal tract, if the examination be not made before the latter have gained their medullary sheath. Superiorly the bordering layer becomes broken up into small bundles by processes of grey matter, and it probably terminates in the reticular matter of the medulla oblongata, between

^a A. Pick. Centralblatt, 1878. 20.

the roots of the hypoglossal internally and those of the eighth pair externally.

IV. *The anterior mixed tract of the lateral column.*—This, the last portion of the lateral column, consists of two sets of fibres, coarse and fine; but whether these are developmentally distinct or not is uncertain. The coarser fibres preponderate in the lower part of the cord, the finer in the upper part. The former pass from the grey anterior horns, and are probably in part derived directly from the anterior roots, partly from the anterior commissure. As they, to a large extent, disappear in the higher portion of the cord, it is almost certain that they pass back again into the grey matter. The share which the nerve-roots take in the formation of this column explains the fact that its size is greatest in the lumbar region, less in the dorsal, and again greater in the cervical. The fine fibres are probably commissural. Superiorly the tract becomes broken up by processes of grey matter, and, without undergoing decussation or other change of direction, passes into the formatio reticularis at the outer and posterior part of the olivary bodies.

V. *The fundamental tract of the anterior columns* is formed of all the white matter between the anterior nerve-roots and the anterior median fissure, with the exception of the anterior pyramidal tract. It even, more markedly than the last tract, varies in size in the different regions, according to the number of nerve-roots and grey matter contained in each part of the cord. It contains both fine and coarse fibres, the latter coming from the opposite side of the cord through the anterior commissure, and being possibly derived in part directly from the anterior roots of the opposite side. Both coarse and fine fibres run only a certain distance in the white substance, and then again bend into the grey matter, between different parts of which, consequently, they are commissural. Superiorly this tract passes into the posterior longitudinal bundle of the medulla oblongata.

In the posterior columns we have to distinguish (VI.) *Goll's column, or fasciculus gracilis*, from (VII.) *Burdach's column, or the fasciculus cuneatus*. In the fully-developed cord these are distinct only in the cervical and upper dorsal regions. In the embryo, however, their distinction is much more readily made out, since the periods at which the two tracts are respectively developed are separated by a long interval. Like the pyramidal and cerebellar tracts, Goll's columns, increasing continuously in size from below upwards, convey fibres from the grey matter of the cord to the

medulla oblongata; it is also probably commissural between different parts of the spinal cord.^a The fibres contained in Goll's column are mostly of the fine variety. Their connexion with the grey matter is effected by fibres passing forwards and outwards into Clarke's columns, and by fibres which pass into the posterior commissure, but which then pass outwards to the same side, and do not decussate. Superiorly the fasciculus gracilis on each side expands into a swelling called the clava. In this is situated a mass of grey matter, known by the name given to it by Mr. L. Clarke as the post-pyramidal nucleus. In the cells of this nucleus the fibres of Goll's column seem, for the most part, to terminate.

VII. *The fasciculus cuneatus* lies between the posterior horn of grey matter and Goll's column. Its size varies accurately in different regions of the cord, according to the mass of the posterior nerve-roots entering at each part. This is explained by its being very largely composed of fibres from the posterior nerve-roots which pass upwards, downwards, and horizontally through its outer portion. Fibres pass from it into the posterior horn of grey matter, to the posterior commissure towards the anterior horn, and, in common with fibres from the fasciculus gracilis, to the posterior vesicular columns. It is not easy to say whether the fasciculus cuneatus is *entirely* composed of posterior roots, or whether it contains commissural fibres also; but, at all events, it maintains a considerable size up to the lower part of the medulla oblongata, where it terminates in the restiform nucleus. It is possible that some of its fibres may pass forwards without entering ganglionic cells, and as fibræ arcuatae run in the formatio reticularis to the olivary bodies; but that any of the fibres of the posterior columns pass *directly* into any of the compact bundles of the medulla is rendered highly improbable by the course which the formation of white substance takes.

The tracts of the spinal cord may thus be divided into two sets:—

1. Those whose section in different parts of the cord varies with the mass of the nerve-roots entering at each part. These tracts are present through the entire length of the spinal cord. They are the fundamental tract of the anterior columns, the fundamental tract of the posterior columns (fasc. cuneatus), the bordering tract of the grey substance, and the anterior mixed tract of the lateral column. They are made up chiefly of fibres derived directly from the nerve-roots before their final termination in the grey matter. Besides these

^a Pierret. Arch. de Physiologie. 1873. 535.

fibres, the tracts in question also contain (most probably) others which serve as commissures between parts of the grey matter standing at different heights in the cord. They contain also fibres which extend from the upper part of the cord to the medulla oblongata—more numerous in the posterior and lateral columns than in the anterior column—and which end chiefly in the cells of the *formatio reticularis*, in the *restiform nucleus*, and (directly?) in the *olivary bodies*. The lateral columns are connected possibly directly with the *thalami optici*. The intimate relation of the tracts of the first class, on the one hand, with the oblongata, on the other, with the peripheral nerves, gives them their special systematic character.

2. Those tracts whose section (number of fibres) increases constantly from below upwards. Two of these have no fibres which are commissural between different parts of the spinal cord, but all the fibres pass from the cord to the cerebrum and corpus striatum in the case of the pyramidal tracts, to the cerebellum in the case of the cerebellar tracts. The third tract, Goll's columns, has probably similar relations, as far as the spinal cord is concerned, but it differs from the other two, which pass by the medulla oblongata without entering into connexion with ganglionic cells, in terminating (at all events in being interrupted) by the post-pyramidal nucleus. None of these tracts extend through the whole length of the cord. The pyramidal tracts extend, greatly attenuated, to the third sacral root—the cerebellar tract and Goll's column only to the upper part of the lumbar enlargement.

It is owing to this second set of tracts that the entire white matter of the cord increases steadily from below upwards. This increase is most evident in, and chiefly due to, the lateral columns, since in it two of these tracts are contained.

The remarkable observations of Waller, showing that nerves when separated from their centres undergo a peculiar degeneration, have been frequently utilised for the determination of the course of certain nervous fibres or tracts. When a mixed nerve is divided, the peripheral end undergoes degeneration, while the central end remains, except in the immediate neighbourhood of the section, intact. When an anterior nerve-root is cut, the peripheral portion only degenerates; and when a posterior nerve-root is divided between the spinal cord and ganglion, the central end only degenerates. The degeneration consists in a segmentation of the white substance, with division of the axis cylinder; the contents of the

sheath of Schwann become by degrees broken up into a granular mass, which is finally absorbed. The appearance of a nerve so degenerated is so remarkable that its course can readily be traced through plexuses in which the unaltered fibres could not be followed. The cause of this degeneration has been the subject of much controversy. If it were due to functional inactivity of the divided fibres, a mixed nerve, when divided, should not degenerate totally, either in its peripheral or central portion, but the centrifugal fibres of the peripheral segment and the centripetal fibres of the central segment should alone undergo change. If inflammation were the cause, it is not clear why the change should extend only in one direction from the point of injury. Axel Key and Retzius^a have shown recently that the connective-tissue sheath of the nerves constitutes channels in which the lymph moves; that there is no communication between the lymphatics of surrounding parts and the lymph spaces of the nerve-trunks, but that the latter open into the sub-dural and sub-arachnoid spaces of the brain and cord. Engelmann^b has suggested that the degeneration of the peripheral segment of a divided nerve is due to the interruption of its lymph current; but this explanation will not account for the different behaviour of the anterior and posterior roots, respectively, when cut—the peripheral segment degenerating in one and the central in the other, although both are similarly circumstanced as regards their lymph. Further, as will appear presently, Engelmann's theory fails altogether to account for the secondary degenerations which occur in the fibres of the nerve-centres. We are, hence, driven back to the supposition that certain nerve-cells exert a so-called trophic influence on the fibres in connexion with them, and that, when separated from these centres, the fibres are unable to maintain their normal structure or functions, and undergo degeneration, atrophy, and absorption. The nutrition of the fibres of the anterior roots is governed by the cells in the anterior horns of grey matter, that of the posterior roots by the cells of the spinal ganglia.

In 1851 Türck described the degenerations which occur in the spinal cord after lesions of certain parts of the brain or of the cord itself, and showed how these changes might be made use of for the determination of the different tracts of fibres in the nervous centres. His observations, although made by imperfect methods, are, never-

^a Arch. f. Mik. Anat., IX.; and Studien in der Anatomie des Nervensystems u. s. w.

^b Pflüger's Archiv. XI., 447.

theless, so accurate and complete that they have left but little for subsequent writers either to add or to change. When cases were examined in which a destructive disease of old date existed in certain parts of the cerebrum, a degenerative alteration was found to affect the following parts—the lower portion or pes of the crus cerebri, the longitudinal fibres of the pons, the anterior pyramid of the medulla oblongata of the same side, the posterior half of the lateral column of the spinal cord of the opposite side, and a smaller tract on the inner aspect of the anterior column on the side of the primary disease. These tracts in the spinal cord agree in their position, extent, and (as has been recently shown) in their variability,^a with the lateral and anterior pyramidal tracts of Flechsig. The parts of the brain whose lesion causes this descending degeneration are those situated in the so-called motor tract, corpus striatum, lenticular nucleus, optic thalamus, crus cerebri, &c.; but it would appear that if the disease is limited to the grey matter of the optic thalamus, corpus striatum, or lenticular nucleus, secondary degeneration does not occur, and that the indispensable condition for its supervention is, that the white fibres passing from the anterior pyramid, through the pons, pes cruris cerebri and inner capsule to the grey cortex, shall be interrupted in some part of their course.^b

It has further been shown by Charcot and Pitres, contrary to the observations of previous writers,^c that lesions of certain parts of the cerebral cortex are followed by descending atrophy, and that these parts are those whose destruction causes paralysis, and whose irritation causes convulsion of the opposite side—namely, the paracentral lobe, and the ascending frontal and ascending parietal convolutions—while other parts of the cortex when injured do not cause motor phenomena or give rise to secondary atrophy of the cord.^d

Where a limited portion of the spinal cord is destroyed, as by circumscribed myelitis, compression, injury, &c., degenerations extend from the seat of injury upwards and downwards, but involving

^a There is no evidence in support of Türck's supposition that degeneration of the anterior tract follows destruction of one part of the brain, while that of the lateral tract is consequent on injury of another; and, so far as I know, there is no confirmation of Vulpian's statement that disease in the corpus striatum causes degeneration of the posterior part of the opposite lateral column, while the anterior part of the column degenerates in disease of the optic thalamus (*Physiologie du Système Nerveux*, 651).

^b Erb. *Zeimssen's Handbuch*. XI., 2, 347.

^c Vulpian. *Physiologie*, 651.

^d *Revue Mensuelle*, 1877. Jan., Fèv., Mars, Mai, Juin.

different tracts. The descending degeneration is confined to the pyramidal tracts, already described, in the lateral and anterior columns. The ascending degeneration, on the other hand, is localised in a narrow tract lying at the outer portion of the lateral column just in front of the posterior roots, and which can be followed upwards in favourable cases to the restiform body of the same side, and in a band along the inner portion of the posterior column, which, becoming narrow, can be followed up to the clava of the same side where the degeneration ceases. These two tracts, it is needless to say, accurately correspond to Flechsig's direct cerebellar tract, and to Goll's column. In the case of degeneration depending on brain lesion, the change is usually unilateral—in that of spinal lesion, bilateral; but where both sides of the brain are diseased, the degeneration is found on both sides of the cord, and where only one half of the cord is affected by the primary lesion, the secondary change is accurately confined to the same side.

Injury of the peripheral nerves causes no secondary degeneration in the spinal cord, but if the posterior roots of the nerves be compressed or divided between their ganglia and their exit from the medulla, then ascending secondary degeneration of the latter occurs. One of the best examples of this is the famous case recorded by Lange, of Copenhagen,^a in which the cauda equina was compressed by a sarcomatous tumour. Above the point of compression the anterior roots were unaffected, but the posterior roots were completely degenerated, and in the lumbar part of the cord the entire posterior columns were degenerated, while above this the change was confined to Goll's column. The only theory which will account for these degenerations is that already given for the nerves—namely, that they are due to separation of the fibres affected from their trophic centres. The trophic centres for the anterior roots may be said certainly to lie in the large cells of the anterior horns (and it will be remembered that these are the only cells in the cord to which nerve-fibres have been certainly traced). The pyramidal tracts have their trophic centre most probably in the cells of the motor region of the cortex cerebri—a view borne out by the discovery of very large cells in the paracentral lobe, which by their size have some analogy to the motor cells of the cord. From Lange's case, just quoted, it is likely that the trophic centre for Goll's column, as for the posterior roots, lies in the spinal ganglia, while the trophic centre for the cerebellar tract is

^a Quoted by Schiefferdecker. *Virchow's Archiv.* LXVII, 588.

unknown, but may possibly be situated in the cells of Clarke's column. This is probable from the frequent co-existence of disease of these tracts and of the cells of the posterior vesicular column.

The pathological anatomy and course of development of the secondary degeneration of the nerve centres is difficult to study in man, for we usually get the case too long after the commencement of the affection to be able to make out clearly how it has begun. It was for a considerable time believed that this change did not follow experimental lesions of the brain or cord in animals, and Vulpian having failed in his attempts to produce it, and having observed pathological cases in which disease of the medulla oblongata and cord were not followed by degeneration, concluded that the latter condition was not due to separation of the fibres from their supposed trophic centre, but to some peculiar irritative condition which was not readily reproduced in animals, and which was present only in certain cases in man.^a Shortly after this, however, Westphal^b observed secondary degeneration in dogs after injury of the spinal cord; and Vulpian,^c having repeated these experiments, obtained similar results. These degenerations were, however, of much less extent and much less characteristic than those observed in the human subject; and Vulpian, while admitting their value, thinks that they do not overthrow the conclusion he had arrived at before. The occurrence, however, in animals of secondary degenerations in all respects identical with those seen in man is now established beyond doubt, and by their study much valuable information has been gained, not only as to the pathology of the process of degeneration itself, but as to the structure and physiology of the cord. In a large work^d recently published Schiefferdecker gives the results of his observations on dogs whose spinal cords had been divided in the physiological laboratory at Strassburg, in the course of the series of experiments on the functions of the nervous centres in which Prof. Goltz has been for several years engaged. These animals were examined at varying intervals after the operation up to eleven months. In most cases they had completely recovered from the immediate effects of the section, but in no instance was there any evidence of the return of sensitive or motor conduction across the point of division, nor was there ever any attempt at

^a Archives de Physiologie. 1869. 233. Ibid. 661.

^b Virchow's Archiv. XLVIII., 516.

^c Archives de Physiologie. 1870. 520.

^d Virchow's Archiv. LXVII., 542.

regeneration of the cord found *post mortem*. From the large number (above a hundred) of cases observed with this invariable result, and from the inconclusive nature of the contrary evidence adduced by Naunyn and Eichhorst, Schiefferdecker thinks that in mammals restoration of a divided spinal cord never occurs, although he admits that the experiments of Brown-Séquard on the cords and of Voit on the brains of pigeons would appear to show that in birds (and hence probably in lower animals) regeneration of mutilated nervous centres may take place. The importance of such a conclusion in the case of man is evident.

It is not necessary to particularise the regions of the cord found degenerated in Schiefferdecker's observations, as they are almost identical with those already described in the case of man, except that in the lower segment of the medulla, in addition to the anterior and lateral pyramidal tracts, numerous scattered fibres in the anterior and lateral columns were found in a state of degeneration; these fibres could be traced down about three to four nerve roots below the section (which was usually situated in the lower dorsal region), while the pyramidal tracts could be followed much further, about six to eight nerves.

The progress of the degeneration was as follows:—In the first week after the section no change was noticed. After fourteen days the degeneration in the nerve-fibres had manifestly commenced, and was advanced to some distance upwards and downwards. After four to five weeks it had reached its maximum both in extent and intensity, and it was not until after eight weeks that the secondary changes in the connective tissue were manifest. The earliest alterations are thus confined to the nerves, whose white substance undergoes a chemical alteration, and becomes converted into a material which stains in aniline and obscures the outline of the axis cylinder, and which finally breaks up into fragments and disappears, leaving in cross sections round holes in the connective tissue where the fibres had previously been. At this stage a great accumulation of granule cells is noticed in the degenerating tract. These probably play here just the opposite part to that which they performed in the formation of the fibres, carrying away now the fatty detritus of the white substance of Schwann. The vessels also present fatty granules in their walls. So far, the connective tissue is unaltered, but its cells soon begin to proliferate, and it gradually assumes a fibrous and finely punctated appearance, and stains deeply in carmine. This newly formed connective tissue finally contracts,

obliterates the spaces left by the nerves, and causes a considerable diminution in the size of the degenerated part. At the later periods also the granule cells become few, and disappear. From this it is seen that the process starts in the nerve-fibres, and that the changes in the connective tissue are secondary, and due most probably to an irritation caused by the degenerated nervous elements, which act as a foreign body, and excite reactive inflammation.^a This is additional proof that the affection is essentially, in the first instance, atrophic—not inflammatory.

Besides the secondary degeneration, Schiefferdecker describes a traumatic degeneration occurring in the neighbourhood of the section, and sometimes in spots at a distance from it, which differs completely from the atrophic change, but the character of which it is not necessary here to specify.

In animals the secondary degenerations seem to produce no symptoms, but in man there is good reason to believe that the late contraction, which affects the paralysed limbs, whether in paraplegia or hemiplegia, and which is usually accompanied by an increased tendency to reflex spasmodic movements when the tendons are struck or suddenly extended, is due to the descending atrophy in the pyramidal tracts.

It is always found that the amount of degeneration in each tract diminishes in proportion as we recede from the seat of primary lesion, and Schiefferdecker has discovered that (at all events in the case of the ascending degenerations) this is not uniform, but that at intervals sudden diminutions take place in the number of degenerated fibres in the different tracts, and that the points at which these diminutions occur are the same for the posterior and the lateral columns. This remarkable observation points to the existence of separate centres in the cord, into which masses of fibres pass, and are lost.

The facts of secondary degenerations show the existence of definite systems of fibres in the end, and the evidence derived from this source so remarkably agrees with that obtained from the facts of development that the conclusions to be drawn from both cannot admit of question.

^a This irritative process sometimes in men extends to the grey matter, producing destruction of the cells in the anterior horn of grey matter and muscular atrophy. Erb. l. c. 359.

ART. XIII.—*A few Remarks on the Treatment of Pleurisy and Empyema, with Cases.* By D. M. WILLIAMS, L.K.Q.C.P.I., &c.; Physician to Liverpool Hospital for Consumption and Diseases of the Chest.

MR. PRESIDENT AND GENTLEMEN,—In describing the few cases of empyema and pleurisy, which it is my purpose to bring before you this evening, I will avoid needless details of symptoms, and reference to the work of others, with which you are all, doubtless, familiar, and confine myself to such cases as will illustrate my remarks, in order to save valuable time.

I will give but a brief history of the first case, as it has been already reported in full in the *British Medical Journal*, December 12th, 1874:—

CASE I.—The patient was thirty-eight years of age—a prosperous man of business—and the attack an unusually severe one. The heart was soon pushed to the right side, and the breathing so difficult that I was compelled to tap him on the twentieth day of his illness, removing 108 ozs. of pus, and again, a week afterwards, removing 50 ozs. The odour of the pus was most offensive, and needed, as was remarked at the time, a pathological nose to endure it. I had sought Dr. Cameron's opinion of the case, and he considered the attack secondary, and that probably the lung was tuberculous, as he had never known a case where the constitutional disturbance was so great, unless there had been previous mischief. The condition of the pus seemed to support Dr. Cameron in this opinion, there being no fistulous communication through which air could have gained admission to the pleural cavity. The result, however, was a perfect recovery; and the patient is now the healthiest-looking man in the firm with which he is connected; still, the lung is, to a large extent, bound down.

In this case I washed out the pleural cavity daily for thirty-one days with a weak solution of sulphurous acid, as the patient preferred it to the carbolic acid which I used two or three times. I must not omit to mention, however, that there was no real improvement up to the twentieth day after the operation—the pus being still very offensive; I, therefore, enlarged the wound with a probe-pointed bistoury; and, on washing out the pleural cavity, the wound became suddenly blocked, and I managed to remove two pieces of false membrane (?), horribly offensive, after which the

^a Read before the Medical Society of the King and Queen's College of Physicians, Wednesday, April 3, 1878. [For the discussion on this paper, see page 424.]

whole aspect of the case changed, and the patient slowly but steadily recovered.

CASE II.—A gentleman in Shropshire, aged thirty, who always enjoyed excellent health, got a severe wetting in a heavy shower of rain, and could not change his clothes for more than two hours. Next morning he felt very feverish, with a pain in right side. In a few days there was dulness, which was attributed to pneumonia; the cough became very troublesome, but expectoration scanty and glairy; no blood, nor anything like it. During the fifth week of his illness, he suddenly expectorated a quantity of purulent matter; and it was now considered that a large abscess had formed in the lung and burst. Two medical men who saw him considered the case hopeless, and gave the poor fellow three weeks to live. The patient's brother (a homœopathic physician), having come from a distant town to see him, gave the same opinion, and treated him for a short time with globules of phosphorus, which made the patient so very sick that he returned to the family doctor, at whose request I was asked to see the case. Dr. ——— had candidly confessed that he was completely puzzled; and, when I arrived, requested me to express my opinion freely in the patient's presence after I had examined him.

I found the patient covered with perspiration, wasted by hectic, with a constant cough, and the whole right side dull; no movement in respiration, nor vocal fremitus; the right side seemed smaller than the left; no bulging nor increase of intercostal spaces; on left side the breathing was puerile, but otherwise healthy. This, with the history of the case, made it quite clear to my mind that it was one of empyema, with a fistulous opening into a bronchial tube; but, as this fistula had existed for several weeks without any real improvement, it evidently did not afford that relief the system needed. The case was fully explained to the patient and friends, and a counter-opening made by tapping. The relief obtained, with the assurance that he would, in all probability, ultimately recover—though after a long illness—quite altered the whole aspect of matters, and brought hope and gladness where there was nothing but sorrow before. He steadily recovered; and, as soon as able, removed to the south of England, and was doing well when I heard last from him, twelve months after my visit.

CASE III.—A. W., aged nineteen, was taken suddenly ill, in June, 1875, with cough, &c.; and, after a long illness, was told he could not recover, as he was consumptive. In spring of 1876 he went to Wales, and improved so much that he remained there the whole summer; but was again ill during the winter of 1876 and 1877. In the spring of 1877 he went again to Wales; but this time he caught a severe cold, he thought, and the leading doctor of the place said he could not get well. He now heard of Congreave's specific for consumption (15s. per bottle)

and took it for several months, with such decided improvement that Congreave was considered a great benefactor. He improved so much that he could climb the hills with comfort. But, on his return journey to Liverpool, he spat blood; and I saw him for the *first time*, October 17, 1877.

A tall, dark-haired man, aged twenty-one, very thin; fingers clubbed and nails incurved; cough troublesome. On examination I found, on left side, dulness posteriorly and tubular breathing; in front the ribs were quite flattened, no respiratory sounds could be heard; at apex there was coarse, moist râles. Right side very resonant; breathing puerile, except at apex, where there was slight dulness on percussion, with prolonged expiratory murmur, and more than the usual increase of vocal resonance. It had evidently been a case of pleurisy with effusion—perhaps a case of empyema; the fluid had been removed by expectoration during the two and a-half years of his illness, but at the cost of a flattened side and useless lung, with tubercle commencing in the other lung. He caught bronchitis on November 7th, and died of hæmoptysis, November 9.

I cannot but regret that the nature of this young man's case was not ascertained sooner, as he must have had a good constitution to endure this long illness, and might possibly have recovered altogether had he been tapped in 1875—especially as he had (like Cases I. and II.) ample means, and could have had every help in the way of change of air, &c.

CASE IV. is that of a girl, aged eight years, who had the usual symptoms of acute pleurisy of left side. On the twenty-fifth day of illness the breathing was so laboured that I visited her in the evening, with my neighbour, Dr. Kirk, intending to aspirate her, but deferred the operation until the following morning, at the earnest solicitations of her mother.

The same night I was sent for in haste, as the child was thought to be dying. I found her nearly suffocated by a profuse flow of pus, which she could scarcely get rid of fast enough to enable her to get breath; a fistulous opening had evidently formed, and the pus was being evacuated without my assistance.

She made an excellent recovery; and I found, on examining the chest four months afterwards, that there was not the slightest dulness anywhere, nor any indications of her very severe illness.

CASE V.—November 21, 1877, Dr. Edis asked me to see a child, two years old, who was nearly suffocated by pleuritic effusion. We found the little thing fairly fighting for breath; and the mother said that the child had been ill six weeks; that she had taken her to the Homœopathic Dispensary, the Children's Infirmary, and the East Dispensary, and that they all said the child had bronchitis.

Right side was dull all over, and seemed smaller than left; immovable in respiration; vocal fremitus absent; no widening nor bulging of intercostal spaces. Left side resonant, and breathing very noisy. I introduced a moderate-sized needle of the aspirator, and, with considerable difficulty, removed several ounces of very thick pus. The child was greatly relieved; but the wound closed; so we inserted a drainage tube on the 25th, making the lower and posterior wound about an inch from the angle of the scapula. The pus escaped freely; and the little thing was doing well the last time I saw her, about a week after the operation. Subsequently, however, Dr. Edis informed me, she began to sink, and died from exhaustion, while the chest seemed to be going on most favourably.

I will not now occupy time in recording the histories of cases of pleurisy with serous effusion, but only remark that they usually take from six weeks' to six months' time before the fluid is absorbed, if it has been thrown out in any considerable quantity; but notwithstanding the longest time (six months), such cases have done well in my practice, the lung expanding once more. I feel, therefore, encouraged to treat these with diuretics, iodine, &c.

Nevertheless, as an illness of two or three months is often a very serious matter to the patient—sometimes causing the loss of employment or of business—I have asked myself if there was no way by which we could hinder the effusion of serum, or, at least, its accumulating in any great quantity? When we remember that the sharp stitch in the side in pleurisy, caused by the friction of the two inflamed surfaces, is intended to limit the movement of the lungs, we cannot but feel that Nature has already given us the hint—stop respiratory movements and the mischief is prevented. We have already recognised this principle in peritonitis, synovitis, &c. Here, however, we have to do with an organ whose function cannot be stopped with safety to the patient; but I do think we can sufficiently limit the movements of *one* lung so as to hinder the formation of, at any rate, a large quantity of fluid in the chest; and this I have attempted to do by strapping the inflamed side firmly with plaster, taking care to bring the ends of the plaster to the opposite side of the spine and sternum.

The first result of such treatment is, a great relief from pain; the next is, a subsidence of fever, &c. To say that a few cases have been thus treated successfully is to prove nothing, as it may be reasonably urged that these were slight ones, or only dry pleurisy; a medical friend, however, has, at my request, already

treated eight successive cases successfully. From my own practice I will give only one case:—

CASE VI.—J. B., aged eleven; always a delicate child; had pneumonia two years since, and confluent small-pox six months after. Soon after his recovery from small-pox a large lumbar abscess formed, “which was allowed to burst of its own accord,” leaving a sinus which leads to the *third* lumbar vertebra. He has also strumous disease of the metacarpal bone of little finger, and second and third phalanges of middle finger, and second phalanx of thumb—all on left hand. Add to this a pale face and bones covered with little besides skin, and I think you will admit that he was altogether an unfavourable subject for acute pleurisy.

His illness commenced on Friday, December 28, 1877, and I saw him on the following Sunday evening. He was then crying out from pain in his right side, especially when he coughed; skin hot; pulse 120; friction sound distinctly audible, and the line of dulness seemed higher than what the liver would account for. As it was raining in torrents, I could not send for my usual plaster, but got some soap cerate plaster from a neighbouring chemist, and with this bound him up firmly. In a few minutes the pain was so much relieved that I felt very much gratified; but, as it was not gone, I ordered two drops of laudanum every three hours.

January 2nd.—He felt so much better that he wished to get up; skin cool; pulse 100; cough very slight; no pain; no more dulness; vocal fremitus quite perceptible.

January 4th.—So much better every way that I allowed him to get up, but not to leave his room, and ordered him half-grain doses of quinine three times daily, and cod-liver oil (3ii.) once a day—treating his general state of health rather than the pleurisy.

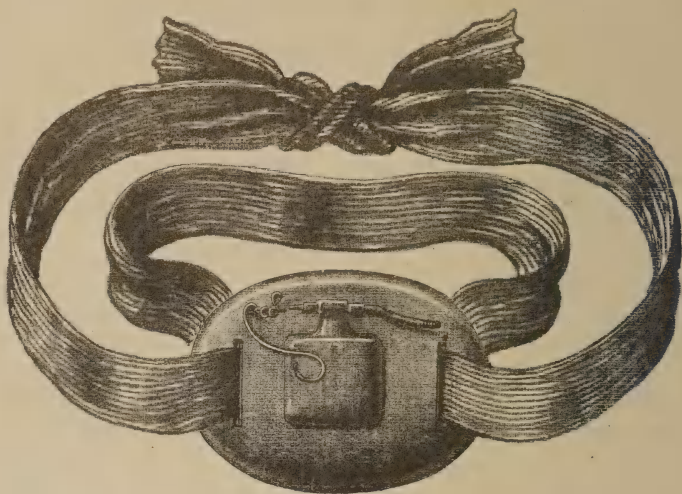
January 7th.—When I entered his room this morning, he was laughing heartily; and, finding all apparently going on very nicely, I dismissed him, only giving his mother directions about him.

I consider this a very valuable case, as the probability is, seeing his age and constitution, that the effusion would have been purulent *ab initio*.

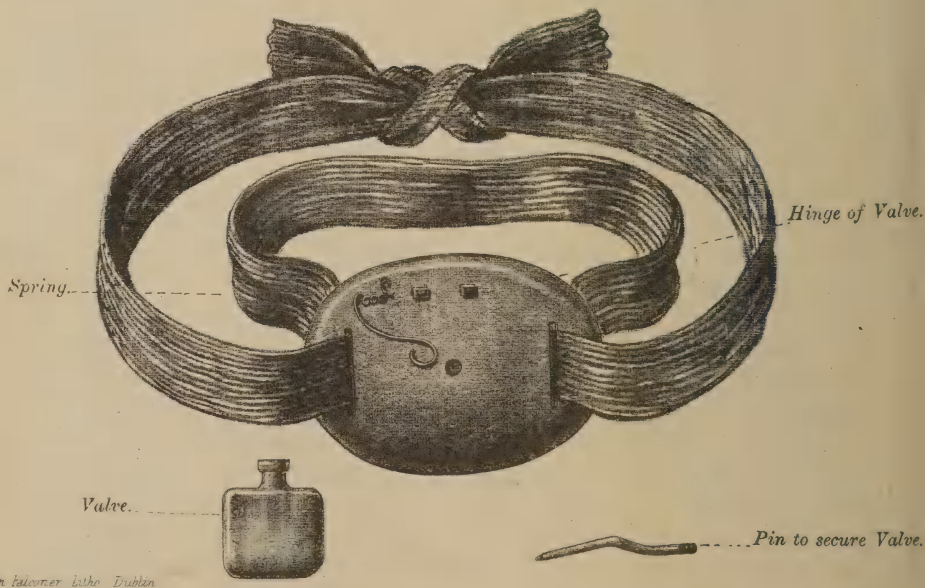
Since writing the above, I have heard that an American surgeon has claimed all thoracic diseases for the domain of surgery, and treats all such by pressure, but not exactly strapping; while Dr. Fred. Roberts has, I find, already advocated strapping.

Again, with reference to empyema, I wish to dwell especially on the condition of the lung after recovery. It is pretty generally admitted that in children under twelve years of age, such cases usually end well—the lung expanding again, whatever may have been the amount of mischief; while in adults this is rarely the case—

CANULA WITH VALVE IN POSITION.



CANULA WITH VALVE REMOVED FOR TROCAR.



The trocar is the size used for hydrocele ; the stem of the canula, however, is made shorter and the head much larger, quite flat and oval, with a slit at each end for tape, a spring to one side, and two fixed eyes for the valve on the upper margin. After introduction, the valve is slipped under the spring with its eye between the two fixed ones and the pin thrust through, making a hinge.

the lung being almost always so damaged as to be unable to support life *alone*. This is surely not a *cure* in which we can take much comfort, however brilliant the recovery may appear to the patient and his friends—as in Cases I. and II. On the contrary, we should not, I think, consider such cases *cured* unless the lung once more occupies its normal situation.

I have asked myself the reason why the lung should expand so easily in children and scarcely ever do so completely in adults; and I think the condition of the ribs in children explains it. Thus, in children the ribs yield rather readily to the atmospheric pressure without losing their spring; consequently, there is not left the same amount of empty space in the chest, when the fluid is removed, as in an adult, while, at the same time, there is a constant suction-power from the tendency the ribs have to resume their natural curve. In the adult the ribs yield much less readily, but eventually become bent in permanently. Now, it has occurred to me that, if we could find some plan of keeping the pleural cavity more or less constantly in a state of vacuum before the ribs bend, without using injurious force, we would help the lung to expand; and for this purpose I have invented a cannula with very delicate valve, which, by acting only one way, allows fluid or air to escape from the chest, but instantly hinders air entering it.

Now, admitting that a patient, with this tube *in situ*, coughs only twice each hour, the force of coughing easily expels fluid out through this tube, while the valve effectually hinders the entrance of air into the pleural cavity. Thus more or less of a vacuum is formed; and in proportion to the amount of vacuum is the aid given to the air rushing through the trachea to expand the lung.

The cannula has been purposely made short, in order that it may be worn until the lung expands pretty freely. It is secured in its position by tapes passed through the slits on either side, and over it should be placed a large cup-shaped sponge, wrung out of a warm solution of carbolic acid (1 to 40), to receive the pus, and a piece of oil-silk to protect the clothes, and a lightly applied bandage to keep all in position.

Seeing, then, that pleurisy, if detected at the commencement, may be greatly relieved, and effusion to any great extent hindered, by firm strapping, it becomes exceedingly important to detect it early. There is, of course, no difficulty in doing this when effusion has already taken place; but, previous to this, I consider it no easy matter to do so in children; and I am not at all surprised that

Case V. should have been overlooked at the dispensaries. The pain in the side and friction-sound, upon which we depend so much in the adult, are of but little use in dealing with children, and I have adopted the following plan:—Suppose a child two years old; very cross and feverish, with a slight cough, and these symptoms not explained by any other disease. I wrap the child loosely in a blanket, and, having put it sitting on its mother's knee, wait a minute or two; presently it coughs and immediately cries. Now put it sitting upright, and watch carefully; next time it coughs you will notice it shrinks to one side. Place a warm flannel on that side, and let the mother keep up firm pressure; the child coughs again, but the crying and shrinking are less; and if, on very careful examination, there is slight dulness, and respiration a little feeble, without crepitation, we may be sure that we have a case of pleurisy; and if that side be strapped, the child will probably have a good night, and be much better next morning.

Supposing effusion to have already taken place (and the patient be a child), the fluid should be removed at once, *if the constitutional disturbance is great*, as in all probability it is purulent—serous effusions giving comparatively little indication of their presence, unless the quantity of fluid is great—indeed, I have been often surprised at the small amount of constitutional disturbance where I have found a large quantity of fluid, in both children and adults. If a doubt exists, the needle of the hypodermic syringe may be used to explore, as recommended by Dr. Barlow and Mr. Parker; but in an adult I would seek to be guided entirely by the constitutional disturbance, which, I think, is much greater where the effusion is purulent.

If, in a child, a fistulous communication with a bronchial tube has already occurred, and pus is being freely expectorated, I would first strap the side and wait a few days, as this seems to be Nature's best and most successful mode of removing pus from the cavity of the pleura in children (while fistulæ through the thoracic parietes almost always end badly). Should the relief be insufficient or the strength giving way, I would then tap the side; but, in the case of an adult, I would tap at once, notwithstanding that a fistula had occurred, as I have not seen a single such case do well without tapping.

The pleural cavity should be freely washed out with a weak solution of carbolic or sulphurous acid, or iodine; and if, after a week or two, the pus remains fætid, the wound should be enlarged

with a probe-pointed bistoury, in order to give exit to any pieces of false membrane or clots of blood which may be decomposing, as in Case I.

There is nothing remarkable in these cases; their interest lies rather in the fact that we are all more or less frequently called upon to treat them. Nor is there much that is new in the treatment advocated, except it be the valved cannula, which I hope some present will put to the test.

In conclusion, I desire to say that it is in no carping spirit that I have referred to the failure of others, as it would ill become me to do so—but simply to illustrate the difficulties which, after all, surround a subject with which we are all supposed to be quite familiar.

ART. XIV.—*Note on a Congenital Band stretching across the Origin of the Aorta.* By ROBERT SAMUELS ARCHER, M.B., Dublin; Assistant-Physician West Derby Union Hospital, Liverpool.

THE specimen to which this note refers was procured quite by accident from a male aged thirty-nine years, the cause of whose death was the subject of a coroner's inquiry.

Having slit up the aorta and left ventricle in the usual manner, I was much surprised to find a band extending right across the aorta. This band was situated just above the line of insertion of the aortic valves, and when the vessel was closed must have flapped up and down in the blood current, as it lay quite loose and slack when the cut surfaces were approximated. Arising by an expansion of about half an inch wide, from that part of the aortic wall which lies just above the junction of the posterior and left lateral semilunar valves (its origin running in a diagonal direction from below, upwards and backwards), it gradually became narrower till it reached its insertion at the junction of the posterior and right lateral cusp of the valve. Its direction thus represented a cord, dividing the circumferential area of the vessel into two unequal arcs, the anterior containing the right and left semilunar valves, and the posterior the posterior valve. It was apparently composed of exactly the same kind of tissue as the valves, which were in every respect normal and competent.

I have searched rather extensively in anatomical and medical literature for an account of a similar abnormality, but up to this

without success. It appears to me if this case is not quite unique, it certainly must be one of extreme rarity.

I regret extremely that, owing to the short time the man lived after admission, there was no clinical observation made of the heart's sounds over the aortic area; but I should fancy there must have been a loud, and probably musical, systolic bruit, possibly followed by a muffled diastolic murmur.

I have no doubt whatever that the band was congenital, as I cannot imagine any diseased state of the vessel producing it. The heart and aorta appeared in all other respects to be quite normal.

The band was doubtless developed together with the semilunar valves, and may be regarded as an irregular and supernumerary cusp.

HOT MUSTARD BATHS IN INFANTILE PNEUMONIA.

At a recent meeting of the New York Medical Journal Association (*N. Y. Med. Record*, Mar. 9, 1878), Dr. Weber read a paper strongly advocating the claims of hot mustard baths in the treatment of pneumonia occurring in children. He propounded the following theory regarding their mode of action:—There were two agents in the baths, both of which acted upon the surface—1. The mustard, which was an irritant to the skin; and, 2. The hot water, which dilated the cutaneous capillaries, thus assisting to increase the quantity of blood thrown to the surface of the body. The increased blood supply to the surface relieved in a great measure the congested pulmonary circulation and the overloaded heart. The baths stimulated the nervous system and also favoured the interchange of gases in the blood through the skin. The advantages claimed for the bath were, that it was easily prepared; the materials could be readily procured; its action was prompt; there was no danger in applying it. The bath was prepared by adding half a pound or a pound of ground mustard to a baby-tub of water, having a temperature ranging from 100° to 105° F. The child was placed in the bath, and, while there, the surface was to be thoroughly rubbed until the skin began to look red, usually from seven to ten minutes, then taken out, wiped dry, and put into a bed previously warmed. No ill effects had been seen by allowing the genitals to remain unprotected. The bath might be repeated as often as every three hours.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Lectures on Surgical Pathology and Therapeutics. By DR. THEODOR BILLROTH. Translated from the 8th Edition. Vol. II., pp. 543. London: New Sydenham Society.

THE most prominent idea in the reader's mind will be that with a little more system the work of the distinguished Professor of Surgery at Vienna might have been made an invaluable one. Its merits are occasionally conspicuous—its faults, or rather its omissions, almost equally so. As lectures on general pathological conditions, the style is clear, concise, and practical; as descriptions of special changes produced in the tissues by definite diseases, the reader will often find them scanty and one-sided. There are several diseases which one would wish had been omitted from the index altogether, so slender is the amount of information about them contained in the work. Of the justice of our complaint we may particularise the accounts given of glanders, hydrophobia, and delirium tremens. The present volume opens with a chapter on hospital gangrene and erysipelas. While not denying the possibility that the first may depend on epidemic miasmatic influences, Billroth is persuaded that it is due to specific causes quite independent of pyæmia. For its treatment he finds painting with tincture of iodine or acetate of alumina answers very successfully. Spontaneous erysipelas, the cause of which is so frequently attributed to cold, he is entirely sceptical about. He is doubtful whether it ever develops except from a wound, or from some pre-existing cause of inflammation. As to the nature of the poison of erysipelas, he thinks it is probably a direct material in the form of dust which can infect a wound at any stage, and which clings especially to sponges and surgical dressings. He has frequently observed patients who, on the same morning, one after another, and in the same operating theatre, and under the same conditions, have undergone surgical operations, have one and all developed erysipelas without any retention of secretions, although the patients have been taken to different parts of the hospital widely separated from

each other. In this manner it may become endemic, attaching itself to the clothes of the surgeon, the beds, or the walls of the wards. With regard to the origin of septicæmia, he neither admits that the excitors of septic fermentation are cocci, nor does he allow that the poison is generated in the blood itself from matters that are not in themselves poisonous. From the fact that decomposing organic matters produce many substances—*e.g.*, sulphuret of ammonium, butyric acid, leucin, &c., which, injected into the blood, act more or less septicallly—he infers that the septic matters are already formed in inflamed and gangrenous tissues, and pass into the blood as efficient poisons. In tetanus the only pathological change in the cord he mentions is a development of fresh connective tissue noticed by Rokitsansky. His own investigations gave only negative results. He omits entirely any reference to the researches of Lockhart Clarke, and the gelatiniform effusion in the cord first noticed by that observer, which *en passant* we may add have been elaborately confirmed by Dr. Bookey, of Steevens' Hospital.

Twelve lectures are occupied with chronic inflammations of the soft parts and of bones. Ulcers are discussed in a very practical manner; they are divided according to their appearance into erethetic (congestive), fungous, callous, gangrenous, and sinuous varieties: those symptomatic of constitutional dyscrasiæ are the scrofulous, lupous, scorbutic, and syphilitic. For the cure of lupous ulceration it is recommended that the floor of the ulcer be scraped with a small sharp-edged spoon, and when the hæmorrhage has been checked that caustic potash be applied under chloroform. On the pathology of rickets and osteomalacia there is no new light thrown; for the treatment of the first-named disease orthopædic contrivances are considered unnecessary. For the removal of ganglia, subcutaneous puncture with Dieffenbach's tenotome and scratching of the inner wall of the sac is the method adopted when the wall is too thick to be burst with a blow. There is an excellent description of chronic rheumatic arthritis under the ill-chosen name of *Malum Coxæ Senile*. The chapter on Aneurism is very disappointing—atheromatous degeneration is described as occurring at "advanced age," whereas in these countries, at least, aneurism is rather a disease of the middle period of life.

The remaining portion of the volume is devoted to the pathology of tumours, and it will be the unanimous judgment of surgeons that this is by far the most valuable part of the work. He accepts Virchow's simple division of the anatomical nature of tumours into

simple hypertrophy when the enlargement is due only to an abnormal increase in volume of the individual elements, and neoplasia when new elements are deposited among the old ones. The new element formation becomes homœoplastic if the indifferent cells deposited develop into tissue homologous with the mother tissue; heteroplastic if they develop into something not homologous. As to the origin of these indifferent formative cells, Billroth seems unable to arrive at any fixed decision. His earlier impression was that they were the offspring of connective tissue-cells; next he was led to consider them emigrant white blood-cells that had escaped from the vessels into the tissue; and, lastly, following out Arnold's observations, he considers that protoplasm completely converted into tissue again degenerates into a granular condition, and forms within itself nuclei and begins to proliferate: a "budding out" (*Sprossenbildung*) of tissue analogous to the "budding out" of a cell takes place; then when the granular protoplasm has differentiated itself into cells, the formation of new tissue results. Hence the doctrine of Schwann, according to which all tissue results from cells, is not prejudiced, though the doctrine "*omnis cellula e cellula*" is thereby considerably modified. While he holds that we cannot pretend to a classification of tumours any more than of diseases generally, he follows the anatomical principle of division by proceeding from the tumours composed of simple tissues to those which consist of more complex structure. Accordingly, fibromata, lipomata, chondromata, and osteomata form the first group. Next come myomata (of whose existence in the strict sense of muscular fibre-celled tumour he expresses considerable doubt), neurómata, on which the splendid monograph of the late R. W. Smith might have been consulted with profit, and angiomas. The sarcomata are divided into eight varieties—besides the round, spindle and giant-celled forms with which we are tolerably familiar, reticular (the myxomata of Virchow), alveolar, pigmented, villous, and plexiform (adenoid) varieties are enumerated. Lymphomata, papillomata, adenomata, and cystomata lead up to the last group on the list—the carcinomata. After showing how unreliable was the supposed cancer-cell as a means of diagnosis, and how apt the alveolar structure was to be confounded with reticular sarcoma or lymphoma-nets, Billroth proceeds to establish his position that "the anatomical development alone now serves to determine what is to be called a cancer." Most of the modern pathological anatomists and surgeons according to him agree in calling only

these tumours true carcinomata which “resemble in construction the true epithelial glands (not the lymphatic glands), and the cells of which are derived from true epithelia.” It is to be regretted that the woodcuts which illustrate the work have been so coarsely executed. Many of them professing to represent histological appearances are such as no microscopist has ever seen, and are barely worthy of being called diagrammatic. Fig. 146 is by mistake termed a myoma instead of a myxoma. Other clerical errors are not infrequent—*e.g.*, Velpeau, in the list of authors referred to, flourished, it is said, from 1759–1867, &c., &c. But, in spite of many little blemishes and several serious omissions, Billroth’s great work will be a welcome text-book of pathology to all lovers of scientific surgery.

Lectures on Medical Jurisprudence. By FRANCIS OGSTON, M.D.
 Edited by FRANCIS OGSTON, jun., M.D. Pp. 663. London:
 J. & A. Churchill.

AFTER the voluminous handy-book of Messrs. Woodman and Tidy, published last autumn, the present work might at first sight seem rather superfluous; but the impulse given to medico-legal study by some recent *causes célèbres* has created a demand for popular treatises on this subject. If the objects aimed at—accuracy of examination and unanimity in medical evidence—can be attained, no one will complain of the multiplicity of authors. The Penge murder case has especially directed public attention to the weak point in the scientific education of practitioners—the deficiency in applying the rules of inductive logic to the testimony given by their senses. From a Scottish writer we should expect very practical applications of the Baconian pursuit of truth, and in the present instance our expectations are not disappointed. Dr. Ogston, the Professor of Medical Jurisprudence and Medical Logic in the University of Aberdeen, has aimed—firstly, at supplying Scottish practitioners with a work containing forms of medico-legal procedure peculiar to that country, and, secondly, at training the legal jurist in any of these countries to methods of observation that will attach to his evidence in the witness-box a weight which is now too often lacking. The first chapter contains a brief account of the modes of legal procedure in criminal cases in Scotland, England, France, and Germany. In one very important respect Scotland possesses a decided advantage over the rest of the United King-

dom. Except in some particular classes of State and political offences, and in a few cases which have drawn on themselves much public attention from their rarity and importance, the English and Irish law recognises no public prosecutor, the prosecution being left to the individual who has sustained the injury, or to his friends. North of the Tweed a public officer conducts the prosecution at the public expense, whether it be in the highest Court where the Lord-Advocate or his deputy performs that function, down to the Petty Sessions where the Procurator-Fiscal prosecutes before Justices of the Peace in cases that fall within their decision. In other respects the difference is more phraseological than substantial.

Dr. Ogston, more than any other legal jurist, seems to keep the witness-box constantly before his eye. The fullest directions are given to the medical practitioner, not only as to the matter but the manner of his evidence. The utmost wiles of the cross-examining counsel are anticipated and detailed; but we believe common sense and a spirit of candour are the real auxiliaries which will recommend scientific testimony to judge and jury. In two chapters on Age and Sex there is an excellent table of the periods of ossification of the bones of the skeleton, and a full account of the varieties of hermaphroditism. On Personal Identity the author's knowledge verges on the skill of the detective. To give an instance, lithograph drawings are given of the impressions left by the foot in the acts of standing, walking, and running. Sexual offences occupy the next eight chapters—that on Criminal Abortion might have been more full, especially in reference to the drugs used for this purpose. There is good reason to believe that this crime is becoming much more common than it was a generation ago. The section on Infanticide, we have no hesitation in saying, is the best part of the work. No other author has discussed in such detail the ways in which the death of the infant may be effected, and the proofs that it has been born alive. In Insanity the classification of Ray is adopted with slight change, lesions of the mental faculties after their development being divided into mania and dementia, mania being either intellectual or moral, as it affects mainly one or other of these sets of powers. The author cannot be said to have either much excelled or fallen short of his predecessors in treating this, the most difficult, question in the whole of legal medicine. Wounds, Homicide, and Death, occupy the remainder of the book, with the exception of some chapters on General Toxicology, the study of particular poisons being wisely left for chemical specialists. The

directions given how to make a *post mortem* examination, and what to look for, might have been more explicit. The safety of the examiner, however, is not forgotten, and, with a national thoughtfulness, he is advised, before setting to his work, "to take a dram."

It is only justice to the author to add that the work is no mere compilation. While frequent references to the opinions of Tardieu, Caspar, Bayard, Montgomery of Dublin, and others, occur, we find that very many of the cases illustrative of the theories adduced are taken from Dr. Ogston's own observation as a medical jurist. As his experience has extended over a period of nearly fifty years, there is an originality imported into the book which would in itself prove a great recommendation. Were it only for the valuable additions these cases make to the literature of the subject, Dr. Ogston's volume would be a useful contribution to forensic medicine.

RECENT WORKS IN DENTISTRY.

A Manual of Dental Anatomy, Human and Comparative. By CHARLES S. TOMES, M.A. London: J. & A. Churchill. Pp. 400.

THIS admirably-printed book has been constructed for the use of students by Mr. Charles S. Tomes, Lecturer on Dental Anatomy at the Dental Hospital, Leicester-square, London. The compiler has brought into prominent notice throughout the volume all the available facts bearing on the subject of Odontology which have been laboriously collected by Professor Owen, Mr. John Tomes, Professor Flower, as well as Kölliker and Stricker. Some original researches by the author in a subject in which, he says, nothing, or next to nothing, has been done—namely, the development of the teeth of fishes and reptiles—are also briefly described.

The book consists of fourteen chapters, in which the following subjects are treated with Mr. Tomes' well-known conciseness and accuracy:—The dental tissues; the development of the teeth in fishes, in reptiles, and in mammals; the teeth of fishes, batrachia, reptilia, monotremata, edentata, cetacea, ungulata, sirenia, hydra-coidea, proboscidea, rodentia, carnivora, insectivora, chiroptera, primates, and marsupalia. In addition to illustrations belonging to papers which have been published by the Odontological Society of Great Britain, Wilson's Anatomy, and Frey's Histology, upwards

of one hundred figures have been specially executed for the work from original drawings and specimens. To those who are interested in the subject the manual must prove useful, as having in a collected form much of the recent additions to our knowledge that must otherwise be looked for in original memoirs, not always accessible. The book is brought out with all the care and accuracy so characteristic of Messrs. Churchill, and the illustrations are printed with great skill.

Notes from a Dentist's Case-Book. By FELIX WEISS, L.D.S.R.C.S.; M.O.S., &c. London: J. & A. Churchill. 1877.

THESE slight notes by Mr. Weiss have been collected and reprinted from the *British Journal of Dental Science*, and contain a few interesting cases of hereditary peculiarities in teeth, reflex affections caused by diseased teeth—such as perverted sense of smell, and deafness. We append, as a specimen of the author's style, a case of hæmorrhage after tooth extraction—a serious and distressing condition which demands prompt and vigorous treatment on the part of any medical attendant:—

“I was called up at about 5 o'clock in the morning, in the month of June, 1865, to see a young lady, aged about fourteen, whom the messenger represented was ‘fast bleeding to death.’ Upon entering her chamber I was somewhat astonished at the scene presented to me. The patient was lying on the bed in a semi-comatose state, her feet and her head being surrounded with ice, and her mouth nearly filled with coagulated blood. The different members of the family were grouped around, but so overwhelmed with grief as to be incapable of affording any assistance. By degrees I was able to glean the following history of the case:—The girl had had a tooth extracted somewhere in the country; but upon her returning home some hours after, the bleeding had recommenced. At about 4 o'clock in the afternoon, a general practitioner residing in the neighbourhood was sent for, who—as far as I could learn—had tried to arrest the bleeding by the application of a mouth-wash. Three hours later, this not having had the desired effect, he was sent for again, and he applied something to the surface of the gums, but did not succeed in arresting the bleeding. At 12 o'clock at night he paid a third visit, and then stated that the case was a very unusual one; that he had done all in his power to check the hæmorrhage; that if the bleeding continued they had better be provided with some ice, which they should apply to the mouth; and if that was not successful, to the feet also. Becoming much alarmed, the girl's mother, at 4 o'clock in the morning, once more sent for

their medical adviser, who replied that he could not do anything more, and advised the application of the ice to the head and feet.

"Such were the particulars of the case told to me by the surrounding relatives; and I learned also that the patient was a strong muscular girl, and accustomed to a good deal of out-door exercise. The first thing I ordered was the immediate removal of the ice, both from the feet and the head, and got the patient between the blankets; at the same time directed one of the family thoroughly to rub her feet, using a little brandy which happened to be at hand, in order to stimulate the return of the blood to the feet. I then proceeded to make a careful examination of the mouth, and found that the first upper molar on the right side was the tooth that had been extracted. The blood appeared to be of fair coagulating power, but I had a good deal of difficulty in getting the patient to allow me to clear out the mouth. I then found that the bleeding was not very considerable, although evidently the girl had already lost a large quantity. As she had retained the tooth, and the fangs were pretty straight, I determined in the first instance to cover them with a small piece of lint wetted with the tincture of the perchloride of iron, not having any of the solution (which I like better) with me, and to drive the tooth forcibly into the socket. I mention these particulars, as I know that this method of proceeding has been advocated by some, but I must say in this instance it was not successful; indeed, I should never advocate using any but a single-fanged tooth as a plug, excepting perhaps a lower molar, where the fangs are short and perfectly straight, but certainly not in the case of an upper molar, where it has the three fangs divided. I should prefer using lint or matico-leaf firmly packed in all double or triple-fanged cases.

"As the blood continued to ooze, more particularly from the palatine socket, I removed the tooth, and taking some long narrow strips of lint moistened with tincture of iron, commenced to pack each of the sockets separately. I then took a block of gutta-percha slightly warmed on the surface and drove it on to the lint between the second bicuspid on the one side and the second molar on the other, and brought the jaws together. In this condition I left the patient for an hour, directing that the gutta-percha should be kept firmly in its place and the mouth opened as little as possible. At the end of that period I returned, and upon taking the gutta-percha away found a small quantity of blood which appeared to possess but little fibrine, oozing from the palatine socket. I therefore removed the lint from this cavity but did not touch the other two pellets. Upon taking the plug of lint away the blood could plainly be seen slowly trickling from the base. Remembering one of those valuable lectures 'On the Medical History of the Teeth,' by Dr. Richardson, in which he suggests as a chemical styptic to produce the speediest coagulation, nitric acid, I determined to try it in this case. I first of all syringed out the cavity with water, to which a few drops of nitric acid had been added,

and then applied the nitric acid in the proportion of one part acid to two of water on a pledget of lint, returning the gutta-percha to its place again, and left the patient. When I returned I found she had fallen into a sound sleep, but upon opening the lips I could not see any indication of blood, and her pulse was regular and stronger than I expected to find it. She had no return of the bleeding, and two days afterwards called to thank me for my attention.

“I think in this case we must attribute the unpleasant symptoms that followed the tooth extraction, more to the particular period of life in the patient than to her being of hæmorrhagic diathesis, for a tooth had been extracted some years before and one has been removed since without any hæmorrhage. It must never be forgotten that it is usually more difficult to arrest a bleeding that has continued for some hours, as the blood becomes deteriorated, its volume in the circulation being made up by the water from the tissues.

“The carelessness displayed by some medical men in mouth hæmorrhage makes it imperative that the dentist should clearly understand that the responsibility rests with him in cases where profuse bleeding follows tooth extraction. Very lately I had occasion to remove two loose lower teeth surrounded by inflamed gum, from an old lady’s mouth. When the patient arrived at home, the bleeding returned, and as it had not subsided by night she sent for her usual medical attendant, who prescribed a simple astringent mouth-wash; but the bleeding continuing all night, in the morning, in a very alarmed state, she called upon me. In less than a quarter of an hour the hæmorrhage was thoroughly arrested under proper treatment.”

Illustrations of Clinical Surgery. By JONATHAN HUTCHINSON, F.R.C.S. London: J. and A. Churchill.

THIS, the tenth fasciculus of Mr. Hutchinson’s splendid work on Clinical Surgery, brings the first volume to a close. We are glad to see that the success of it has been such as to justify the continuance of the publication, and we may, therefore, hope to see still further additions from the same pen and pencil to the literature of medicine. The subjects treated of in the present issue are osteitis, pyæmia, and bullet perforations of the skull. As to the question whether, when pyæmia occurs with phlebitis and osteitis, it is caused by these, or that they are with it a common result of blood-poisoning, the author believes the evidence to be overwhelming that the phlebitis is the cause of the rest of the symptoms. He differs entirely as to fact from the assertion that proof of

phlebitis is but rarely found, believing it to be rare, if proper search be made, that no local condition of disease is discovered. He proposes the use of the term phlebitis-pyæmia as applicable to such cases. It may be left to future investigation to determine whether or not there are other forms of pyæmia which are not due to phlebitis. It is very possible that there are rare instances of arteritis-pyæmia, and also that inflammations of the lymphatic channels may sometimes contaminate the blood and supply material capable of producing infective embolism.

Transactions of the Pathological Society of London. Vol. XXVII. 1877. Pp. 471.

THIS volume, illustrated with thirty-four plates and nine woodcuts, maintains the reputation of the transactions of which it is the twenty-eighth volume. Some will read with special interest the communication of E. Klein, M.D., on the anatomical changes of the kidney, liver, spleen, and lymphatic glands in scarlatina of man (with eighteen illustrations). This important paper refers to investigations undertaken for Mr. Simon, late medical officer of the Privy Council and Local Government Board, and will be found in their eighth Report, published *in extenso*.

Exposé Statistique de l'Organisation des Hôpitaux Civils en Danemark. Par P. A. SCHLEISNER, M.D. Copenhagen. 1876. Quarto. Pp. 90.

THIS work, which is a valuable contribution to the literature of hospital construction and statistics, was compiled in 1876 by the Medical Officer of Health for Copenhagen, and was presented by the Danish Committee to the "Congrès d'Hygiène et de Sauvetage," which assembled at Brussels at the end of September of that year.

In the introduction an interesting historical account is given of hospital organisation in various countries, and the author pays a well-merited compliment to England. He says:—"L'Angleterre qui, pour ainsi dire, a enseigné l'hygiène à l'Europe, occupe aussi un rang très-élevé pour tout ce qui concerne l'hygiène des hôpitaux." He adds that, up to a few years ago, almost all English hospitals owed their origin exclusively to private contributions, and that it

was only by the "Sanitary Act" of 1866 that the State was brought into intimate relations with the hospital system of the country. The 37th section of this Act conferred on municipal authorities, for sanitary purposes, the right of establishing hospitals or temporary accommodation for those suffering from epidemic diseases. As regards London, this *permissive* power was defined and regulated in the "Metropolitan Poor Act" of 1867, in accordance with the provisions of which the small-pox hospitals at Hampstead, Homerton, and Stockwell were founded. He shows from the history of hospital organisation that there is in all countries a tendency to construct and maintain—in addition to general hospitals—special epidemic hospitals, which may prove of the greatest use in checking local epidemics by effecting isolation in contagious diseases.

Under the heading "Le Régime Hospitalier en Danemark," Dr. Schleisner gives a very complete description of the origin, funds, number, mode of construction, and general management of the Danish hospitals. In 1876, it appears, there were 100 hospitals in the kingdom; the total number of beds was 5,816. If lunatic asylums and the large hospitals for incurables are deducted, the number should be set down as 93, with 3,773 beds—that is to say, one bed for every 507 inhabitants. In Copenhagen itself there were (in 1876) 9 hospitals, containing 1,853 beds, or one for every 114 inhabitants. In London, according to Dr. Chapman, the corresponding facts were in 1872—78 hospitals, with 7,825 beds, supported by private means, and 3 epidemic hospitals, with 580 beds—or one bed for every 387 inhabitants—the population of London at that time being 3,200,000 souls. As peculiarities of the Danish organisation of hospitals may be mentioned the fact that all hospital physicians are salaried—in Copenhagen compared with most foreign places even handsomely remunerated, that a large staff of assistant physicians and "internes" always reside in the larger hospitals, and that the wards never contain as many beds as may be found in most foreign hospitals—as a general rule only 6 or 12. In Copenhagen, patients, who are admitted to the hospitals, may be divided into four classes:—

1. Those who pay all the expenses of their treatment and nursing.
2. Those who are admitted at a reduced rate—for example, members of societies or of sick funds.
3. Patients whose means do not correspond to their social position, and who have a right to beds endowed by legacies and donations.

4. The poor properly so-called, whose expenses are all defrayed at the public cost.

The section on "Construction of Hospitals" is one of the most instructive in the work. Having adopted the pavilion or the barrack system as alone suitable for epidemic hospitals, Dr. Schleisner proceeds to treat of ventilation and heating. In Copenhagen there are, or are to be, two epidemic hospitals. One, the Öresund Hospital, was erected in 1875, and is intended for the reception of patients suffering from epidemic diseases, including cholera, brought to the town on board ship. It has lately been used and has done good service in an outbreak of contagious dysentery. It is constructed in the form of massive barracks with saddle-roof ("Reiterdach"). Heating and ventilation are secured on the principle laid down by Captain Douglas Galton—namely, that each room shall be heated and ventilated separately. This is effected by means of M. Krarup's ventilating stoves. The hospital contains 32 beds, the cubic space varying from 1,000 to 1,200 feet per bed; but as the hospital grounds are extensive, there is sufficient space whereon to erect tents in case of emergency. Each "barrack" is provided with a bath-room, kitchen, *cabinet d'aisances*, laundry, combustion and disinfecting ovens after the model in the London "City Mortuary." In the "saddle-roof" are movable panes, which turn on a transverse axle. The other epidemic hospital will not be finished until next autumn. It is intended for the admission of patients suffering from small-pox, typhus, and dysentery. It will consist of six double "barracks," one "barrack *d'observation*" (intended for the reception of doubtful cases), containing twelve apartments, each with one bed, and one "barrack" of twelve apartments, each with one bed, for pay patients. The total number of beds is to be 168, and the general plan of construction is almost the same as that of the Öresund Hospital just described. In two barracks, however, at the suggestion of one of the technical members of the municipality, a combined system of heating will be introduced.

The experience of Scandinavian sanitarians has condemned the water-closet system in hospitals. Dr. Schleisner points out the objections to that method of getting rid of fæcal matter. He is of opinion that contagious germinal matter by regurgitation from the drains may frequently find its way into the water-closet, and that the usual system of sewers and drains may often produce a dangerous subterranean mode of propagating contagious particles.

Accordingly the system of movable trenches or tubs (*fosses mobiles, tonneaux transportables*) has been adopted. The model in use is of Swedish origin (*closet Marino*). This appliance is based on the separation of the urine from the solid fæcal mass, whereby the decomposition of the fæcal matter is retarded and the closet is also freed from the foul odour which hangs about water-closets of all kinds.

Statistics of deaths in the Danish hospitals for the five years ending 1875 show that the general mortality from all causes varied from 9·5 per cent. (the highest) to 4·0 per cent. (the lowest). Omitting patients suffering from scabies and syphilis, the mortality varied from 13·4 per cent. to 4·9 per cent. The general mortality in the Copenhagen hospitals was 8·6 per cent., or—omitting cases of scabies and syphilis—11·9 per cent.

An epitome of the organisation of the civil medical service of Denmark brings this highly valuable work to a conclusion. Nine lithographic plates are appended in illustration of the several systems of ventilation which are in vogue in the Danish hospitals. These plates also explain various details, which cannot fail to be useful to hospital physicians and architects.

J. W. M.

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1. *A Villa Hospital.* By FRANCIS VACHER, Medical Officer of Health, Birkenhead.
 2. *Fifty-seventh Annual Report of the Seaman's Hospital, Greenwich (late Dreadnought).*

MR. VACHER'S little pamphlet is a reprint of a paper read in the section of Public Medicine at the Manchester meeting of the British Medical Association. The paper shows how two comparatively valueless villas were converted into a hospital. The paper may serve as a hint to others; but in order to be of particular application elsewhere it would be necessary to obtain two villas on nearly the same plan as those obtained by the Birkenhead authorities, and, what is more important, on similarly favourable terms. Two villas for £500 are seldom to be had. If villas had to be purchased at their full value, we say it would be much more preferable to build a properly constructed hospital. We are afraid Mr. Vacher's Villa Hospital is only a makeshift, and one which unfortunately may serve as a hint to other authorities to provide similar makeshifts.

To Mr. Henry C. Burdett, the energetic secretary of the Seaman's Hospital at Greenwich, is due the concise, and at the same time satisfactory, report of the doings of this useful institution. We are glad to congratulate the managers of the hospital on its continued success, and on having such an able officer as Mr. Burdett. We beg to call particular attention to the statement of accounts which accompanies the report. The medical officers of Dublin hospitals are constantly charged by the non-medical governors with a tendency to extravagance. We would ask such to look at Mr. Burdett's figures, and learn that in a well-managed hospital the cost is an average of £64 per annum per bed occupied. We wish the suggestion made some time since by Mr. Burdett were carried out, that a uniform plan of keeping hospital accounts could be adopted, by which the expenditure of hospitals should be fairly comparable with one another.

T. W. GRIMSHAW.

Ugentlig Oversigt over Sygdomme, Dødsfald, og Fødsler i Kjöbenhavn.
Weekly Return of Diseases, Deaths, and Births in Copenhagen.

THROUGH the courtesy of Dr. P. A. Schleisner, the distinguished Medical Officer of Health for Copenhagen, we have been favoured with copies of the Weekly Returns of Births and Deaths in that city from the beginning of the present year. In addition to his office as "Stadslæge," or Medical Officer of Health, Dr. Schleisner appears to occupy a position—so far as relates to Copenhagen—similar to that of Registrar-General in this country, for the Weekly Returns, compiled by him and published by direction of the Municipal Council (*Kommunebestyrelse*), are exactly analogous to those published by authority of the Registrars-General of England, Scotland, and Ireland.

The Copenhagen Weekly Returns are very interesting, more particularly because a complete system of *registration of disease* has been carried out in the Danish capital for many years. Thus, Table I. in the Returns gives a detailed survey of all cases of epidemic and infectious disease reported weekly to the Medical Officer of Health by every medical practitioner—civil or military—in Copenhagen. As the whole question of compulsory registration of disease is *à l'ordre du jour* both in the United Kingdom and in Germany, this portion of the Danish Returns cannot fail to be

instructive. In Denmark the reporting of the occurrence of cases of epidemic and infectious diseases has long been obligatory on all physicians, and in the Danish metropolis since 1854. At first the undertaking was beset with many difficulties and obstacles, but at present we believe the machinery for a systematic registration of disease works smoothly and well.

Table II. is equally novel and important. It classifies all cases of epizootic affections reported by veterinary surgeons practising in Copenhagen or its vicinity. The necessity for a comparative study of epizootics and of epidemics is recognised by every sanitarian, and great credit is due to Dr. Schleisner for having originated a system of scientific registration of disease occurring amongst the lower animals. He is of opinion that the question whether the contagious form of pneumonia, which sometimes occurs, may be derived from the epizootic pneumonia of the horse, or scarlet fever from a similar epizootic disease, can be determined by simultaneous returns of epidemics and epizootics. He has, moreover, utilised this Table in investigating the contamination of milk from "*aphthæ epizooticæ*" in the cow.

Table III., which specifies the "*Causes of Death*," is very like the corresponding Table in the British Returns. Table IV. sets forth the comparative birth and death-rates in three different portions of the registration district of Copenhagen, having a very diverse density of population; while Table V., communicated by the Danish Meteorological Office, gives (among other things) the temperature of the sea-water of Öresund, the percentage amount of salts contained in it, and the weekly number of "*sunshine-hours*." Table VI. contains a return of deaths from prevailing epidemics in foreign large towns and seaports.

It will thus be seen that Dr. Schleisner's Reports, which are arranged principally with a hygienic scope, are at once exhaustive and novel. Even those to whom Danish is an unknown tongue may peruse them with pleasure and profit, as the principal facts are expressed in French, while the Danish terms for the various diseases are followed by their Latin equivalents.

J. W. M.

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

SAMUEL GORDON, M.D., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, April 3rd, 1878.

DR. GORDON, President of the Society, in the Chair.

Exhibition of Cases.

DR. HAYDEN exhibited to the Society a lad, nineteen years of age, the subject of congenital atrophy, or arrest of development, of the right upper extremity. The contrast, in respect of length and volume between the two upper limbs, was very striking. There was also a very singular curvature of the cervical portion of the spinal column. There was a convex curve towards the right side, and a compensatory curve in the dorsal region on the left side. Dr. Hayden was rather inclined to think that the cervical curve was compensatory of the other. As far as the boy himself had been able to learn from his parents, he was strong, healthy, and well-developed at the time of his birth, and for some time afterwards. About a year after his birth he fell from a table, and his mother noticed that from that time he exhibited weakness of the right upper extremity, and was ill for five or six years, but it was not clear what the character of his illness was. His present measurements, made by Dr. Gunn, were as follows:—In height he was 5 feet $4\frac{1}{2}$ inches. The scapula from the superior to its inferior angle measured on the left—which was the unaffected side— $6\frac{1}{2}$ inches, and on the right $4\frac{7}{8}$; from the superior angle of the scapula to the acromion, 5 inches on the left, and $4\frac{1}{4}$ on the right; from the inferior angle of the scapula to the acromion, $7\frac{1}{2}$ inches on the left, and $6\frac{1}{2}$ on the right; length of left clavicle $6\frac{1}{4}$ inches, and of right $5\frac{1}{4}$; from the tip of the acromion to the outer angle of the humerus, 12 inches on the left, and $8\frac{1}{2}$ on the right; from the head of the humerus to its outer angle, 8 inches on the right; length

of left radius 9 inches, and of the right 9; left ulna $10\frac{1}{8}$ inches, right $9\frac{3}{8}$; left hand $7\frac{1}{4}$ inches, right $6\frac{3}{4}$; circumference of the left arm, at the middle of the biceps, $7\frac{3}{8}$ inches, and of the right, at the same place, $5\frac{3}{4}$; circumference of the forearm, 5 inches below the elbow, $8\frac{1}{4}$ inches on the left, and $6\frac{7}{8}$ on the right; circumference of left wrist, 6 inches, of right, $5\frac{5}{8}$; length of left arm from the tip of the acromion to the tip of the middle finger $27\frac{1}{2}$ inches, of right, $23\frac{3}{4}$. It was apparent from these measurements that the difference in the length of the two limbs was mainly due to the difference in the length of the two humeri. Dr. Gunn had taken the trouble of comparing the lengths just given with the measurements contained in Dr. Humphrey's well-known work, and from the latter it appeared that in a person of 5 feet 6 inches in height, the length of the humerus was 12.54 inches. In the case of the present young man the left humerus was 12 inches, and the right $8\frac{1}{2}$. According to the standard of Humphrey the radius should be 9.46 inches; its length in the present case was 9 inches. The length of the hand according to Humphrey's standard was 7.33 inches; in this young man's case it was $6\frac{3}{4}$. There was great atrophy of the muscles at the shoulder-joint, so much so that one's thumb could be buried in the hollow. His right side was quite paralysed. He had no grasping power in the right hand, but had fair grasping power in the left. A wasting process had gone on in the left upper arm, but not in the left forearm or hand. The right side of the trunk was less developed than the left; and on careful observation it would be seen that the right face and the right side of the skull were also less developed than the left. The lad was fairly intellectual, and had gone through the greater part of the first book of Euclid, and mastered it fairly well. He was sufficiently intelligent to write, but could not use his right hand for that purpose. He had been under magneto-electro treatment for some time. At first the muscles did not respond, but afterwards they did, and he felt that he was getting some little power in his right arm again.

DR. NIXON exhibited a boy, aged eleven years, labouring under cyanosis depending, he believed, on congenital lesion of the heart. The mother and two sisters of the boy had died of phthisis. About three years ago he complained of an attack of pain in the cardiac region, dyspnoea on exertion, and neuralgic headache, and then the cyanotic condition developed itself. A short time afterwards he became subject to fainting fits, and in order to obtain relief from these he presented himself at the hospital. Each of these fits lasted about five minutes. When he was examined on admission to the hospital, it was found that there was a considerable increase in the area of cardiac dulness upwards and to the left side. The sounds of the heart presented no abnormal phenomena; they were simply exceedingly distinct and accentuated. There was no murmur whatever. The temperature ranged from 95° to $96\frac{1}{2}^{\circ}$. The

marked cyanotic condition of the lips and tongue would be observed. Upon examination of the optic discs with the ophthalmoscope the edges of the discs could hardly be distinguished from the retina. The veins were extremely engorged and tortuous, and were like a number of snakes crossing the retina. About three weeks ago a number of purpuric patches developed themselves on the extremities, but these had since disappeared. He had since been subject to attacks of epistaxis. He had, however, no cough, nor any sign of pulmonary lesion, his respiration being normal in both the front and back of the chest. In association with the congenital lesion of the heart a very singular conformation of the left forearm existed, it being considerably shortened, and there being only one bone—namely, the ulna—which terminated in some of the bones of the wrist. He was first treated with peroxide of hydrogen, and afterwards with inhalations of oxygen.

DR. WILLIAMS (of Liverpool) read a paper entitled, “A few remarks on the Treatment of Pleurisy and Empyema.” [It will be found at page 398.]

DR. HAYDEN thanked Dr. Williams for reading his paper before the Society. He alluded to the difficulties connected with the treatment of effusion upon the chest, and observed that medical men had latterly taken heart, and had not hesitated to act upon the dictates of common sense and remove by mechanical means the fluid which was interfering with the breathing of the patient. As to the danger of admitting air into the serous cavity, that bugbear had been likewise exorcised, and now they knew that, so far from aggravating the disease, the admission of air into that cavity really alleviated it, and perhaps started a process of cure. He had some difficulty with respect to the second of Dr. Williams’s cases, in which a fistula was detected before the operation of tapping. They had not been informed as to the ultimate progress of the case, and as to whether the fistula continued, or whether the lung expanded. If the fistula continued he could hardly suppose that the lung had expanded. He could not approve of the treatment of effusion by means of opium; he had never seen any benefit to result from it. He regarded opium as a mere palliative, and believed that so far from being an excitant, it was rather a depressant; so that if the object was to excite the lungs in order to promote the discharge of the fluid, he could hardly suppose that, reasoning *à priori*, opium was a judicious plan of treatment. Having had some little experience of tapping the chest in cases of effusion, he believed that where it was serious, where it was the result of acute inflammation, and where life was threatened from the mechanical pressure of the fluid, after the ordinary means of quickening the action of the kidneys and bowels had been tried and had failed, no more time should be lost, but the operation of tapping should be at once performed.

Dr. HENRY KENNEDY alluded to the unpleasant effects that sometimes resulted from the injection of fluids into the pleural cavity, and asked had anything of that sort occurred in Dr. Williams's experience? He had seen two cases in which the effect of the first tapping was to withdraw fluid of the most foetid description; and thought that fœtor of that sort was connected with the state of the patient's constitution.

Dr. FINNY observed that the treatment of pleurisy by strapping had been longer known in Dublin than Dr. Williams gave them credit for. It was done in the hospital he was connected with in nearly every acute case, and the disease, he believed, was thus often checked. The question was one of obtaining physical rest; and when this could not be procured by strapping, it was necessary to have recourse to opium in order to arrest the circulation. In this way they strove to bring about adhesion between the parts, and thus prevent effusion. Dr. James Little, one of their Vice-Presidents, had published cases of treatment of the early stages of pleurisy by strapping. When the existence of pus was recognised, was it better to tap frequently with the aspirator *in vacuo* or give free vent to the pus at once by incision? According to his own experience he believed the latter course was the best. One reason for leaving the pleural cavity open was that sometimes large flakes of pus would not pass through the aspirator.

The PRESIDENT said he had seen a great many cases of pleurisy with effusion, and had tapped over 25 cases, and, as a general rule, with great success. He had tried strapping the chest in a great many cases; and his experience was that it afforded most relief in cases secondary to phthisis, particularly those in which there happened to be pneumothorax. In those cases it gave immense relief. On the contrary, in cases of an acute type which ran into empyema, it was not so applicable, and, if tried, gave great pain. Many such cases were accompanied with great external and internal inflammation; and there was also external periostitis which would not bear pressure. Cases of pleurisy supervening on phthisis derived sensible relief from treatment by opium. Another point of great interest was, that, as he had more than once seen, when cases of well-marked empyema terminated in fistula, the purulent matter expectorated was the consequence of the fistula that had been formed. On the other hand, where a fistula did not form, the case became one of chronic empyema. That was the condition of affairs which led to such cases being mistaken for phthisis. The possibility of that occurring was originally alluded to by the late Dr. Green in a very able article in a comparatively early number of *The Dublin Medical Journal*. Another important point was as to whether the pleura should be completely emptied at once, or the effusions withdrawn by portions at a time. This was a matter supposed to depend on the patient's health and strength. There was no question that formerly medical men were too much influenced

by the dread of allowing air to pass into the cavity of the pleura. The late Dr. Adams detailed a case in which there was a large empyema, and he completely evacuated the contents of the pleura, using all possible means to prevent the entrance of air. In a few hours the patient died. At first it was thought that the intercostal artery had been wounded, and that the patient had died from hæmorrhage. A close examination showed that there was no wound of the intercostal artery, but that a very large quantity of blood had been drawn into the cavity of the pleura.

Dr. WILLIAMS, in reply, thanked the Society for the attention with which they had listened to his paper and for the remarks that had been made concerning it. He had used strapping very largely and with much benefit in cases of phthisis. He was surprised to hear the remarks of Dr. Finny as to the frequent employment of strapping in Dublin, and was glad to hear such testimony in favour of its utility. He had not seen the least harm result from the washing out of the pleural cavity. In the first of his cases he washed it out thirty-one days before he ventured to make the wound large; and he now believed that if he had made a large wound in the first instance the man would have got well in half the time. When the first opening was made, 108 ounces of most offensive pus were removed. He concurred with Dr. Kennedy that the smell of the pus depended on the constitutional state of the patient.

The Society then adjourned.

CHOREA IN OLD AGE.

M. CHARCOT has at present, at the Salpêtrière, two cases of chorea in men over seventy years of age. One has had it ten years, the other twelve. The affection is to be distinguished from the so-called *chorea senilis* with which it has no affinity—it is precisely the chorea which attacks children, except that it resists all treatment and is incurable. The movements are a little slower, and the variety of the grimaces rather less. There seems to be no relation between it and rheumatism, nor in the autopsies of several cases that came under his observation was there any lesion of the heart. It begins generally suddenly after excitement or some other emotional cause; and most cases are accompanied with impairment of the mental faculties. The malady as a rule does not tend to shorten life; one old woman, however, who came under M. Charcot's observation died with typhoid symptoms shortly after a violent attack of the affection, and an old man, the half of whose body only was affected, succumbed, a rapid rise in temperature preceding death.—*Le Progrès Médical*.

S. W.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

FORTIETH ANNUAL SESSION.

Saturday, 9th February, 1878.

S. M. MACSWINEY, M.D., Vice-President, in the Chair.

The Late Dr. Churchill.

DR. M'CLINTOCK.—Before we proceed with the ordinary business, I have to ask your permission to move a resolution. It is always distressing to have to refer to the death of a fellow-member. It is peculiarly so on the present occasion, when the resolution has reference to one who was endeared to me by over twenty-five years of warm friendship. I believe that it would be quite unnecessary for me here to dilate on the professional character of the late Dr. Churchill. His reputation as an obstetric writer was of the highest degree. For nearly a quarter of a century his works on midwifery and the diseases of women and children have been text-books universally read and referred to; have been in the hands of students and practitioners alike, and have been republished in America and translated into European languages. As a practitioner Dr. Churchill was personally known to us all. There are few members of the Society who have not had at one time or another to benefit by his great experience and matured judgment. As a gentleman and a member of the medical profession he was, I need hardly say, of unsullied reputation. He maintained the honour and dignity of the profession in every relation of life and in every position. In addition to this, as most of you know, he was a man of cultivated understanding and of very enlarged Christianity of heart. I need say no more to commend the following resolution to the acceptance of the Society:—"Resolved: That we place on record our sincere regret at the decease of a distinguished fellow-member, Dr. Churchill, who, from the foundation of the Society until the time of his death—embracing a period of nearly forty years—never ceased to take a warm and active interest in its welfare; and who, by his own professional eminence as well as the moral weight of his character, contributed in no small degree to the reputation of the Society."

DR. DENHAM.—I have very great pleasure indeed in seconding the resolution so ably proposed.

The CHAIRMAN.—Gentlemen, you have heard the resolution which has been proposed by Dr. M'Clintock and seconded by Dr. Denham. It is the only tribute which this Society can now pay to the memory of a former President who was so endeared to every member of the Society; and it has been proposed in terms so appropriate and graceful that nothing need be added.

The resolution was passed *nem con.*

Specimen of Head of Fœtus.

DR. W. J. SMYLY.—The specimen I have the honour to lay before the Society this evening is the head of a fœtus presenting a spoon-shaped fracture and depression on the left side of the frontal bone. These injuries are sufficiently uncommon from any cause, but are especially rare when the fracture takes place by the unaided contraction of the uterus. They are generally caused by instrumental delivery when the bone is dragged past the prominence of the sacrum. In the present case it was the woman's seventh pregnancy. She had had an abortion in 1876, and she again became pregnant last year. Her last menstruation was on the 7th of April, and the child was born on the 23rd of December, so that the child was an eight months' fœtus. Labour commenced on the 22nd of December, at four o'clock, a.m., and the membranes gave way at three o'clock the following afternoon. She was seen shortly afterwards by one of the pupils, who on examination found that she was in the second stage, the os being fully dilated; but the head was still above the brim. With that exception there was nothing abnormal. I examined her again at nine o'clock in the evening, and found three or four loops of the cord in the vagina, but quite pulseless. The child being dead I considered it advisable to let the labour go on of itself. At half-past one o'clock on the morning of the 23rd no advance had been made since four o'clock the previous afternoon. The head at this time had not entered the brim of the pelvis. While Dr. Malone was making an examination with one of the intern pupils of the hospital he suddenly felt the head come down with a jerk, and the child was immediately afterwards born. He had some difficulty in extracting the shoulders. The child was of course dead, but apparently had not been so for a very long time. There was no scalp tumour, nor were there any signs of putrefaction or of uterine maceration. The fracture is situated in an uncommon position, for these fractures usually occur in the anterior portion of the parietal bone and rarely on the frontal bone. The cause of the fracture was that, the pelvis being deformed, the bi-parietal diameter of the head could not enter the conjugate diameter of the pelvis. This caused the head to be displaced to the left side, and in the descent of the forehead the frontal bone came in contact with the promontory of the sacrum and was fractured. The reason of the fracture being so far forward is the small size

of the head. The displacement of the head to the left side of the pelvis left room for the cord to descend on the right side.

Specimen of an Ovarian Cyst.

DR. ATTHILL.—I have to exhibit an ovarian cyst which was removed by me to-day from a patient in the Rotunda Hospital. The patient was a girl aged about eighteen. She came under my care some four months ago, and at that time I diagnosed the existence of an ovarian cyst of small size, evidently unilocular and free from adhesion. Taking into account the condition of the patient I was indisposed to operate, and postponed doing so from time to time. I was unwilling to risk the life of the patient unless the symptoms should become aggravated. Of late she began to suffer considerable pain of a rather perplexing character, for though the pain was referred to the tumour there was no symptom whatever of peritonitis. There was no pain on pressure, and there were no rigors. This frequent occurrence of pain, coupled with the urgent wish of her relatives, decided me to operate. The operation in its steps was exceedingly simple and easy. The cyst was exposed without difficulty and proved, as we had diagnosed, to be unilocular and free from adhesions. I was surprised, on introducing a trocar, to find that it contained only pure pus. We removed it; and a very small quantity—a drachm or two—escaped into the abdominal cavity. The cyst was then turned out, and the pedicle secured by a silk ligature, and returned into the cavity of the abdomen. The cyst has not been opened yet. The fact of its being in a state of suppuration is very satisfactory, for although the escape of pus into the abdomen somewhat increased the risk, it was quite evident that the patient would have succumbed before long if we had not operated on her. The cyst is exceedingly small, and the quantity of pus in it was not more than a pint and a-half. It was nearly free from solid matter. The patient up to the present is in a very satisfactory condition. Her pulse is about 110 and her temperature about 101°.

Adjourned Discussion on Transfusion.

The discussion on Dr. M'Clintock's case of transfusion, communicated at the preceding meeting of the Society, was then resumed.

DR. ROBERT M'DONNELL.—Before the discussion proceeds I wish to show and explain my transfusion apparatus. As the operation is one of great emergency, the case should contain every requisite, and mine is even provided with candles, which are often needed in the houses of the poor. The only thing you have to ask for is some warm water and an ordinary jug. The first thing to be done is to secure the arm of the person who is to give the blood. Bleeding is now-a-days so rarely performed, that gentlemen who require to perform transfusion have frequently to begin by learning to bleed—a great many students of the present day having never seen it done.

There is an India-rubber band which is strapped round the arm, and causes sufficient pressure. As the blood flows into the vessel provided to receive it, it is stirred with a clean glass rod. In a very short time the blood commences to coagulate; and as soon as it has coagulated we strain it through a piece of muslin or a clean pocket handkerchief which has been previously scalded, into a perfectly clean vessel. The blood is then poured into the pipette at the top or sucked up into it. An India-rubber tubing is used to facilitate matters. When the pipette is filled, it is placed in a jug of water heated to 100 degrees. It will remain there at the proper temperature ready for use; and there will be nothing to confuse the operator during the second part of the operation, which requires some little delay, and also involves care. It is not necessary to use so much as fills the pipette; the most successful operations have been accomplished with about three-fourths of the quantity it contains, or from six to eight ounces of blood. The India-rubber tube enlarges at the centre into a bag; and by squeezing this a considerable impulse can be given to the tube, and the pulsations of the heart imitated. The next part of the operation involves the difficulty of finding a vein in the patient, who is generally quite bloodless. In a fattish person, although we may not be able to see the vein, we will be able to feel it by drawing the point of the finger across the elbow, and it will sink to where the vein is. The skin is pinched up, and an incision made with a small sharp-pointed knife. When the vein is hooked up, a small opening is made in it sideways. The probe point, which extends a considerable way beyond the eye of the instrument, is passed into the vein; and the blood then comes through, and we know that all the air has been expelled. If the weight of the blood be not sufficient to force it into the vein of the patient, blowing into the instrument or squeezing the bag of the India-rubber tube will force it in.

DR. MACAN.—I have listened with great pleasure to the successful case of transfusion which has been brought under our notice by Dr. M'Clintock, especially as he was before, to a slight extent, a disbeliever in the operation, and not without cause. From what I have heard from Dr. M'Donnell, I understand that those cases of transfusion do best that require the least blood—in other words, those cases are most successful in which 4 or 5 ounces of blood are put in to replace 40 or 50 ounces lost. The object, therefore, is not to replace the blood lost, but to supply a stimulant to the brain and heart. The only attempt mechanically to replace lost blood has been in cholera cases, in which large quantities of saline solution have been injected. There are different degrees of anæmia. In one of these the centres are paralysed from sudden loss of blood, but there is still a sufficient quantity of blood remaining in the body to carry on life if it were properly distributed. It is in these cases that the treatment has proved most successful, whether it has been by the subcutaneous injection

of ether, by the application of Esmarch's bandages to the limbs, or, lastly, by the transfusion of blood. There is no danger in the mere transfusion of a large quantity of blood. Experiments on animals have shown that twice or three times the quantity of blood contained in an animal can be injected into it without causing any dangerous symptoms. But it has been found that the injection of a large quantity is, I will not say impossible, but useless; for, as Dr. M'Donnell has stated, the most brilliant results have been obtained from the transfusion of a small quantity of blood. Auto-transfusion, or the bandaging of the limbs, is most applicable to cases in which a sudden loss of blood has paralysed the nervous centres, and the blood remaining in the body is badly distributed; and in such cases this method has been attended with brilliant results. The veins of the abdomen receive a large quantity of blood, and when the limbs are bandaged and friction applied to the abdomen the blood is sent on from those veins to the heart, and the person at once rallies. If we want to see transfusion carried to an extreme we have to go to Germany, where, in some cases, transfusion is practised before giving chloroform, in order to counteract the anæmia of the brain caused by the chloroform. Again, blood lost in an operation may be at once transfused into the patient, so that, if transfusion should become an operation commonly practised, I believe there will be nothing to prevent the blood lost by a woman in *post partum* hæmorrhage from being returned into her body after it has been defibrinated. I think there is still room for doubt as to whether defibrinated blood be the best for use or not, especially in the cases of puerperal women. Defibrinated blood has not the same effect in causing the contraction of the womb and the arrest of hæmorrhage that blood which has not been deprived of its fibrine has. The keeping of the patient's head very low—as is always done in such cases in the Rotunda Hospital—as well as the subcutaneous injection of ether and the use of Esmarch's bandages, have the effect of redistributing the blood and giving the requisite stimulus to the nervous centres; when cases go beyond all these means, I would attempt transfusion itself. As to the best means of transfusion, it is not for me (not having had any personal experience in the matter) to pronounce an opinion; but from knowledge derived from books I would be inclined to think that transfusion from artery to vein, with two small glass tubes between the vessels, would be the most practicable mode of transfusing. As to transfusion becoming an operation to be brought generally into practice I am very doubtful. The greatest obstetricians on the Continent have come to the conclusion that, even if it be a good operation, it is still only capable of being used in very few cases.

DR. KIDD.—I have so often expressed my opinion as to transfusion that I hesitate about rising now, especially as my experience of it has not been successful. I believe, however, that that was not the fault of

the operation, but of the time at which it was performed. I have myself, in some two or three instances, performed transfusion, but in cases which I believed at the time to be utterly hopeless, and which therefore did not afford a fair trial to the operation. I am sorry I did not hear Dr. M'Clintock's paper; however, I have considered the whole question many times very carefully. I believe the method proposed by Dr. M'Donnell to be the best and safest that has yet been adopted. I believe that the danger of the formation of coagula in transfusing blood which contains the natural proportion of fibrine is so great that it adds very materially to the risk of the operation. We may look on it as a perfectly established physiological fact that defibrinated blood is amply sufficient to restore the patient and carry on the functions of life. All the most advanced physiologists maintain that fibrine is excrementitious matter, and not an essential part of the blood. In my opinion, transfusion, in order to be useful, ought to be adopted at an earlier stage than it has been hitherto. I believe the reason why it failed in the cases in which I tried it was that it had not been employed early enough. In those cases I was called on when it was too late to have any chance of saving the life of the patient. There are so many cases now on record in which the operation has been performed without any evil consequences ensuing that I should not hesitate about performing it.

DR. DENHAM.—I have very little to add to what has been already said. We are all of opinion that this operation is frequently necessary and often desirable, and that great risk is often run in postponing it. We should seldom wait for the purpose of having recourse to Esmarch's bandages and other means of that sort. I am satisfied that the operation is a perfectly safe one in the hands of such men as Dr. M'Donnell; and I feel assured that the success of it will be rendered much more certain if it be undertaken earlier than has been hitherto the practice.

DR. KIDD.—Permit me to add one word. In the application of auto-transfusion it is important, not merely to lower the head, but also to raise the body. Our practice in the Coombe Hospital is to elevate the pelvis and also the feet and legs. In cases of extreme hæmorrhage we always use some solid body for the purpose of so placing the patient as to make an inclined plane from the heels to the head.

DR. DENHAM.—In the Lying-in Hospital we raise the bed altogether, which is the simplest way.

DR. MACAN.—That is what I meant when I spoke of lowering the head.

The CHAIRMAN.—It should be borne in mind that this method of transfusion is an especially Irish practice, as contrasted with the mode at present associated with the name of Roussel. An impartial person acquainted with both of those methods, will I am sure, arrive at the conclusion that the weight of argument and evidence is altogether in

favour of the adoption of Dr. M'Donnell's, or the Irish method. It is characterised by an extreme of simplicity and a minimum of difficulty, consistent with the accomplishment of the object in view, and recognises the latest and most authoritative expression of physiology on the subject of the part which fibrine plays, and the mode in which it is to be regarded as an element in the blood. The comparative amount of success which has hitherto attended the performance of Dr. M'Donnell's operation in Ireland, and the absence of any dangerous consequences in the cases in which it was performed, furnish additional arguments in favour of its adoption. The absence of any clots—such as almost every operator who has adopted Roussel's method, and with whose writings on the subject I am familiar, has stated that that method has a tendency to produce—would alone be a sufficient ground for giving one vote in favour of Dr. M'Donnell's, on the Irish method. With reference to auto-transfusion and the beneficial effects of elevating the body, I may recall the fact that more than thirty years ago the late Dr. O'Ferrall, while engaged in removing an enormous scrotal tumour, the history of which is published in *The Dublin Hospital Gazette* of the time, adopted with my assistance, as his clinical clerk, the same method of getting rid of a large quantity of blood which was in the tumour.

DR. KIDD.—I believe the principle of auto-transfusion was first described by Dr. Wise, of Middleton, in the county of Cork, in a paper contributed to *The Dublin Quarterly Journal*, soon after I became editor of that journal.

DR. M'CLINTOCK.—One point to which Dr. Macan has alluded has occasionally agitated my mind, and it is this—viz., the propriety of resorting to transfusion whilst any bleeding is going on. I do not myself think it should be tried until after the hæmorrhage has been completely suppressed. We know that bleeding will sometimes continue as long as there is any blood in the body, and only stop with the extinction of life. I cannot understand how transfusion could bring about the contraction of the uterus and arrest hæmorrhage if the direct injection of perchloride of iron failed to do so.

DR. M'DONNELL.—I need hardly say that I am much gratified at having made a convert of so sound a practitioner as Dr. M'Clintock. I entirely concur in the advisability of what he has said as to not undertaking the operation unless hæmorrhage has entirely ceased. To attempt in such a way to save the life of a patient who was still bleeding would be like pouring water into a vessel without a bottom in it. Such a proceeding would be only calculated to bring into disrepute an operation which is most useful in proper cases. I failed to catch exactly whether Dr. Macan is in favour of the use of defibrinated blood or not. He spoke, in conclusion, of the advantage of direct transfusion of blood from an artery into a vein, and considers that such blood is better calculated to

bring about contraction of the uterus, and prevent hæmorrhage. But I think such an operation is attended with considerable danger. Before the law prohibited me from making experiments which are useful for the purpose of saving human life, I made a good many experiments involving the withdrawal of blood from living animals, and I was struck by the rapidity with which blood coagulates in the small tubes. Let me briefly go through the several steps necessary to be taken in that operation of bringing blood directly from an artery into the vein of a sinking patient. You have to get, say, the radial artery, and to put a small glass or silver tube into it. This is very difficult on account of the contraction of the arterial coat when it is exposed to the air. But supposing you get it in where you have a small nozzle with an eye at the end of it, it is very difficult to put it into a small tube. This is one of the advantages of my instrument having a probed point a considerable way beyond the eye. But supposing this to be accomplished, you have next to put the cannula into the arm of the other person. During the time occupied in getting the blood to pass from one to the other, a clot would be formed, so that the proceeding would commence by the throwing in of an embolus. That, to my mind, renders the attempt at direct transfusion of blood containing fibrine full of danger. Of all the apparatus that I know for transfusing with fibrine, the simplest and best is that of Dr. Aveling; but even in the case of that instrument the fibrine is apt to clog the valves. One end of it is put into the arm of the person giving the blood, and the other into that of the person who is to receive it, and there is also a bag in the India-rubber tube to accelerate the passage of the blood by squeezing; but there is still danger of a clot. And when we set against that, that for the purpose in question the fibrine appears of little use, I still adhere strongly to the opinion that it is better to use defibrinated blood. Let us consider what are the principal objects of transfusion. They are three in number. First, it serves a mechanical purpose in supplying something for the heart to grasp—a *point d'appui* for the circulating system; secondly, it is a stimulant; and, thirdly, it gives food. Defibrinated blood appears to accomplish all these objects. The patient who has suffered from bleeding is completely bloodless, and the heart has nothing to catch upon. That want would be, to a certain extent, supplied by the introduction into the system of any fluid. In the next place, the red corpuscles of the blood supply a stimulant. A case occurred in Jervis-street Hospital of a girl who was dying of tetanus. She was totally unable to swallow, and it was impossible to administer nourishment by enemata. She constantly exclaimed—"I am dying of hunger." It was a pitiable case, and had run on for a month from the receipt of the injury which produced the tetanus; and so I determined to try transfusion. It was accordingly performed. It did not exercise the slightest effect in controlling the spasms, and the patient's life was

not ultimately saved, but it entirely took away her feeling of hunger and thirst. That showed that the transfusion in her case performed the part of a good meal; and there are many cases in which such a remedy may save life. We should remember, above all things, that the cases in which we are called upon to perform the operation of transfusion are for the most part cases of terrible emergency, and in which the life of the patient is in great jeopardy. We hope to grapple with a few of these cases, and to save them, which will be a great matter; and I cannot help thinking, from what I have seen, that we are justified in coming to the conclusion that if it be performed with ordinary dexterity, it is free from danger. It has not been attended—and I would again dwell on this—with any ecchymosis, bloody urine, or internal hæmorrhage, which have been described in some German clinical lectures. Furthermore, it is not attended with any pain to the patient, who is generally insensible.

Saturday, 9th March, 1878.

Dr. DARBY, President, in the Chair.

Milk Fever. By ARTHUR V. MACAN, M.B., M.Ch., Dub.

THE expression “Milk Fever” has now been in common use among practitioners of midwifery for many centuries. Its exact origin is enveloped in obscurity, but the fact of its existence is certified by the most ancient traditions, and by the accumulated evidence of successive generations of medical men, by whom it has been recognised and described.

We must not, however, be surprised in the present age, when the general attitude of the scientific mind towards all ancient theories and beliefs seems to be one of universal scepticism, if milk fever has been indicted as an impostor, and called upon to plead before the bar of the profession for its very existence. Those who know how important a part it has played, and still plays, in the pathology of the puerperal state, will not think it waste of time to pass in review the theories that have been held with regard to it, and to examine the grounds on which its very existence has lately been called in question. This I purpose to do to-night; and will embody in this paper the results I have myself obtained from an analysis of the temperature of more than 400 (446) puerperal women, who were admitted into the Rotunda Lying-in Hospital during the time I had the privilege of being Assistant-Physician to that institution.

The first theory of its production that I have been able to find is in a book published in the year 1655 by an Englishman named Willis. He held that women, during their menstruating age, are subject to the periodical production in their blood of certain highly fermentable particles, the retention of which in the body, if the woman be not pregnant,

gives rise to various disorders. During pregnancy, however, this menstrual material need not be cast out, for a nutritive substance, or milk, is being continually deposited in abundance about the uterine portion of the placenta for the nourishment of the fœtus. As soon, however, as the child is born, this menstrual material or milk is got rid of through the lochia, and afterwards by the secretion of milk by the breasts. Should the milk not be secreted by the breasts, it is determined to the uterus, where it accumulates and is discharged as a whitish humour—the lochia alba sive lactea. If the milk be unable to escape—that is to say, if it is obstructed in the breasts, and also in the womb—it is retained in the system, and gives rise to all sorts of evils, and among them to milk fever (Meigs “On Childbed Fever,” p. 53).

This doctrine of the obstruction of the milk was held by our renowned countryman, Smellie, and was the cause, I think, of his failing in his chapter on Milk Fever to diagnose accurately between it and the more serious puerperal affections, for, while he admits that milk fever, when unaccompanied by uterine complications, is not so dangerous as the other fevers, and much easier to cure, still he says:—“Most of the complaints incident to women after delivery proceed either from the obstruction of the lochia in the uterus, or of the milk in the breasts.” (McClintock’s ed., Vol. I., p. 405.) And further on, when speaking of the distension of the breasts, he says:—“But if the woman catch cold, or is of full habit of body, and not very abstemious, the tension and pain increasing will bring on a cold shivering, succeeded by a fever, which may obstruct the other excretions *as well as those of the breast.*” From these passages it is plain he held the doctrine that the obstruction to the milk was the cause of milk fever, and we think we may infer from the following passage that he had an indistinct perception of what was afterwards elaborated by Puzos into the theory of the dispersion of the milk. Thus, at page 413 he says:—“If in spite of these endeavours the fever proceeds for some days, the patient is frequently relieved by critical sweats, a large discharge from the uterus, miliary eruptions, or *loose stools mixed with milk, which is curdled in the intestines.*”

The next mention of it is by Dr. John Maubray, who, according to Denman, was the first public teacher of midwifery in England, and who published a book entitled “The Female Physician” in 1724. He taught that milk fever occurred in almost all puerperal woman about the third or fourth day, that it was caused by the milk being converted from the womb to the breasts, and that it resolved itself about the ninth day in sweat. It was not as a rule dangerous, but many circumstances might turn it into a putrid fever, and as he quaintly expresses it, “a latent *cacochymy* may also easily dispose it to corruption.”

In 1756 Dr. Young, who was then a professor of midwifery in Edinburgh, and who had been a lecturer for some time previously, considered

milk fever as due to the distension of the breasts, and says nothing about the milk being thrown upon them from the uterus.

Almost simultaneously with the appearance of Smellie's work in England in the year 1751, Dr. Astruc, Physician to the King of France, published his *Midwifery*, in which he speaks of the uterine milk which is formed in the lactiferous vessels of the womb. About the second or third day the mouths of these lactiferous vessels become closed by the contraction of the uterus, and the uterine milk is forced to regurgitate on the blood, and to join the milk of the breasts, from which it does not in the least differ. Milk fever is partly caused by distension of the breasts, and partly because, to use his own words, "the milk by stagnating longer in the blood grows ascendent, and hence acquires a tendency to thicken it, which occasions a shivering or cold fit, more or less strong." This is followed by a hot fit which lasts from 15 to 20 hours, or even a day and a half, and generally terminates in profuse sweating, but may occasionally run on into a continued fever. He states that, in his time, milk fever was much more common than it had been previously, when it was the custom to hire women to suck the breasts as soon as they contained any milk. When he wrote, this custom had gone out of fashion, women thinking that it softened the breasts and so spoiled their shape. It seems probable, from the views already quoted, that this custom had its origin in the idea that the first milk secreted was nothing but the impure humours which were thus finding a way to escape from the body.

A few years later, Puzos published his treatise on Milk Deposits ("*Sur les dépôts Laiteux*"), in which he developed his celebrated theory that all puerperal disorders, such as peritonitis, pelvic abscess, phlegmasia dolens, and puerperal mania, were caused by a metastasis or dispersion of the milk from the breasts to the intestines, the uterus, the lower extremities, and the brain. At the same time, however, he gives most minute and accurate directions as to the diagnosis of these affections from milk fever. His arguments in support of this theory were short and conclusive. He held that during pregnancy the milk was formed, and circulated in the vessels along with the blood, but was drawn to the uterus to nourish the child as long as it was there. As soon as it was born, the milk had to find some way to escape from the system, either by the vagina or by the breasts, in the urine, or in the stools, which latter, he said, were whiter than at other times. It was obvious that if the milk was prevented from thus making its escape, it must accumulate in the system; and, that it did so accumulate, was proved by numerous *post mortem* examinations, in which it was found in curdled masses in different parts of the body. The influence of this theory, which is obviously nothing but a modification of Willis' theory already mentioned, of easily fermentable menstrual particles being present in the blood, was long felt on the Continent, but was soon refuted, though by no means universally abandoned, in England; and as a neces-

sary consequence, the theory of milk fever in this country also underwent a change. Thus, in 1784, a small book was published by the celebrated Mr. Charles White, of Manchester, entitled—"An Inquiry into the Nature and Cause of that Swelling, in one or both of the lower extremities, which sometimes happens to Lying-in Women; together with an Examination into the propriety of Drawing the Breasts of those who do, and also of those who do not give Suck." In the first part he shows, by a number of cases, that the milk-leg of puerperal women is not due to a deposit or redundancy of the milk. In the second, he condemns the practice that was common at that time of drawing the breasts of women who did not intend to nurse, which custom evidently had its origin in the idea that if the milk were allowed to accumulate in the breasts, it would be thrown back on the blood, and thus give rise to various disorders. And then, with a candour and courage worthy of all imitation, he adds:—"I must acknowledge that, at the time I wrote my 'Treatise on the Management of Pregnant and Lying-in Women,' in 1772, I had a different idea, and *even believed in the doctrine of depositions or translations of the milk*; but further observation and experience have convinced me of my error, and I am not ashamed to recall what I said on that subject" (p. 68). He gives the following quotation from the lectures of John Hunter:—"If the patient is not to suckle her child, many things are recommended to be applied to the breasts. In Ireland they have them drawn, supposing that if the milk is locked up it will produce fever. . . . I always prefer leaving the breasts to nature, and letting the milk come into them, and either run out or be carried back into the constitution, to be afterwards discharged by stool, urine, &c. . . . In general, however, nothing is required but patience for a few hours, and the case always ends well; and I do not believe there is any risque from giving up the milk and leaving it to nature. It is very natural, I must allow, that a woman should suckle her own child; but many women are so delicate and nervous, that after teasing themselves and their child in endeavouring to do this, they are obliged to give it up in a few days, or a week; and I believe this is not attended with the least danger. I reason from facts, for there is hardly one of my patients that suckles her child, and yet they recover much better, and are much stronger after lying-in, than those who do. For the omniscient Author of nature, who has contrived everything in the most proper way, foresaw that children would sometimes be born dead, or die soon after birth; and has therefore taken care that the life of the mother should not depend on that of the child, but that the milk should be carried off without doing any harm."

We may gather from this passage, and also from the views enunciated in a book published in 1781 by Dr. E. Foster—who, as we are informed on the title page, had formerly been a teacher of midwifery in the city

of Dublin—that the theory of the obstruction of the milk was at that time held in this city.

White's views were very similar to Hunter's. He says that if the breasts are not drawn—"Women recover faster, and much trouble is saved by this means, and the breasts are absolutely prevented from gathering. I have proved beyond the possibility of doubt that it does not occasion the swelling of the lower extremities; and I am equally convinced that it does not occasion either the puerperal or miliary fever, and that the milk fever is slighter, and of much shorter duration, than when the breasts are drawn; nor do I see any inconveniences that can attend this mode of treatment, but what will attend the drawing of the breasts, in as high a degree at least" (p. 74).

However, though as early as 1784 Hunter and White had both repudiated the theory of the translation of the milk, and almost, as a necessary consequence, the allied theory of milk fever being due to the throwing back of the milk on the blood, still we find this theory in vogue in Scotland as late as 1803. For in the "*Edinburgh Practice of Midwifery*," published in that year, the chapter on Milk Fever is little more than a reprint out of Smellie's work, published more than fifty years previously, in which, as we have already seen, all puerperal disorders are put down to the obstruction of the lochia in the uterus, or of the milk in the breasts. Even in the fifth edition of "*Denman's Midwifery*," published in 1815, the author thinks it necessary to mention as groundless the idea that puerperal fever is in any way due to the milk, though he thinks an intimate consent between the uterus and the breasts is abundantly demonstrated by "the transition of the humours from one to the other." This is the latest trace I have found of the theory of Puzos, and since that time up till quite recently, though writers here and there have said that the fever is due to the pain and over-distension of the breasts, still the prevailing idea among British authors has, I think, been that it is connected with the physiological action of the gland accompanying the first secretion of the milk. On the Continent, however, as early as the year 1820, Professor Carus stated that so-called milk fever was in reality due to a number of separate causes, such as cold, emotion, errors in diet, irritation of the breasts and nipples, and of the internal genitals; and Schroeder, in his "*Midwifery*," mentions a number of circumstances, unconnected with the secretion of the milk or septic influence, which may cause fever in the puerperal woman. He thinks that normally the first secretion of the milk is accompanied with a rise in temperature of a few fifths of a degree Fahr., though the temperature may rise as high as 100.76° without any pathological process being present. In some cases, however, the heat and the distension of the breasts are very great, and the temperature may rise to 104° or 105° . For this condition he reserves the name "milk fever," though he acknowledges that symptoms of local inflammation

in the breast are present, and that a more suitable name for it would be *non suppurative parenchymatous mastitis*. Professor Winckel,^a of Dresden, objects most strongly to the term "milk fever" as quite unscientific, and as propagating false views as to the pathology of the affection. He holds that when the temperature exceeds $100\cdot76^{\circ}$, there must be actual inflammation of the breasts present—a statement which corresponds very nearly with those of Schroeder and Schram, the chief point in dispute being the propriety of calling this fever "milk fever." In France the question of the existence of milk fever has been recently investigated by M. Chantreuil.^b He concludes that any increase in temperature which amounts to as much as 1° Fahr., must be put down to the trauma of the genital tract, and not to the secretion of the milk. This is also the view held by Prof. Halbertsma, of Utrecht, and by Dr. G. Wilds Lynn,^c of Philadelphia.

In my own cases the temperature was as a rule taken only once daily—viz., between 3 and 5 o'clock, p.m.; but the patients were constantly under observation, and I was at once informed if any of them were attacked with a rigor or shivering, so that the cases of fever which could have escaped notice are very few. Of the total number of 446 cases, I find that 23 are, from various causes, useless for this investigation. In 114, or about 27 per cent., of the 423 cases remaining, the rise in temperature during the whole period the women were under observation did not exceed 1° Fahr. Further, taking, as Wunderlich does, $100\cdot4^{\circ}$ as the limit of a subfebrile temperature, we find that in 197 cases, or 46·5 per cent., the temperature never rose to fever height. If we now analyse the 226 cases in which the temperature did rise to fever height—i.e., above $100\cdot4^{\circ}$ —we find that in 32 cases, or a little over 7 per cent. (7·5 per cent.), the only cause that could be found for such elevation was a painful and distended condition of the breasts. The temperature rose on an average to $102\cdot4^{\circ}$; and in 21 cases, or 65 per cent., the fever lasted more than twenty-four hours, and in 1 case for as long as five days. It is but right to mention that in a considerable number of cases (13) great distension of the breasts did not cause fever, and in a few cases the temperature was even below normal. In some of these cases of fever from over-distension of the breasts the pulse seemed remarkably slow in comparison to the temperature. The most striking instances of this condition were:—A temperature of $104\cdot5^{\circ}$, with a pulse of 108; $103\cdot7^{\circ}$, with a pulse of 92; $103\cdot75^{\circ}$, with a pulse of 104; 104° , with a pulse of 112; and $100\cdot5^{\circ}$, with a pulse of 80. This is the exact opposite of their relation in cases of septic infection, where the pulse is as a rule too high for the temperature. This fact may help us in the diagnosis; but, judging from

^a Pathologie und Therapie der Wochenbeter. 2nd Ed., p. 405.

^b Archives de Tocologie. Aug., 1874.

^c Philadelphia Med. Times. May, 1874. A resumé of this paper and that of M. Chantreuil may be found in the Irish Hospital Gazette. Sept. 1st, 1874.

my own experience, I would say that mere fulness of the breasts is but a very uncertain diagnostic sign between so-called milk fever and the more serious puerperal disorders. For I think that if the breasts are already full when the woman is attacked by puerperal fever, the fulness may last for twenty-four hours, and it may be much longer till the milk disappears entirely. Indeed, I think it doubtful whether a slight attack of cellulitis, or transitory septicæmia, has any influence on the secretion. Shortly stated, the conclusions I have come to are :—

1. There is no rise in temperature necessarily accompanying the first secretion of the milk.

2. Pain and distension of the breasts may cause fever; but this fever differs greatly from that generally described as milk fever. It comes on somewhat later and lasts much longer; for, while milk fever is said usually to terminate in from 8 to 24 hours, this fever, in 65 per cent. of the cases, lasts more than 24 hours, and does not, I think, so frequently terminate in profuse sweating.

3. The pulse in these cases is often much slower than the temperature would seem to warrant.

4. In cases of fever, during the puerperal state, the presence of full breasts is not sufficient justification for at once diagnosing the case as one of milk fever.

Having thus as it were disposed of milk fever, we are naturally led to inquire what has now-a-days become of ephemera, weid, miliary fever, and the seven forms of hydrosis described by Blundell. These are, I think, but the various expressions of surgical fever, or of septicæmia in all its varied forms. This is very nearly the idea put forward by Dr. McClintock, in his note to Smellie's chapter on "Milk Fever," in which he also embodies his views about milk fever. At p. 411 he says:—"For my part, I believe that many of the cases of rigor followed by pyrexia, commencing about the third, fourth, and fifth day of childbed, and which from being unattended by any marked uterine pain or tenderness, are commonly described under the euphemistic name of weid or milk fever, are really not milk fever at all, but have a septicæmic origin. Extreme distension of the breast may cause some headache and febrile disturbance; these cases are rare, but in my judgment they are the only proper examples of this milk fever."

Before concluding I would wish to draw the attention of the Society to the view held regarding the production of milk fever by Dr. Carpenter, one of our greatest authorities on physiology. He says:—"There is reason to believe that if while the process (*i.e.*, lactation) is going on it be suddenly checked, the retention of the material in the blood, or the re-absorption of the secreted fluid, is attended with injurious consequences. The accompanying fever is partly due, no doubt, to the local disturbance, but in part also there seems reason to believe to the re-absorption of the

milk into the blood; this cannot but be injurious, since although but little altered the constitution of milk is essentially different, especially in regard to the quantity of crystallisable matter (*i.e.*, sugar) which it contains.”^a This view is probably founded on the discovery by Blot, in 1856, of sugar in the urine of nursing women, which has been looked on for years by Spiegelberg as diabetes, due to the re-absorption into the circulation of the sugar of the milk contained in the breasts. This view has lately been shown to be correct by Hempel.^b He found that the amount of sugar in the urine was in direct proportion to the activity of the breasts. It is, therefore, a sort of physiological diabetes, and since there was no fever present in any of the cases examined by him, I think we may conclude that so-called “milk” fever is not in any way caused, as Dr. Carpenter seems to think, by the absorption of the sugar of the milk into the general circulation.

The PRESIDENT.—This interesting paper affords an ample field for discussion on many points. Dr. Macan has given us the views of the old pathologist who supposed that there were humours in the blood; and he has also given us an account of the septicæmia of more recent times which some of us do not believe in, and pointed to the physical and pathological conditions involved in those cases. One very remarkable fact related by him is, that the pulse of the patient and the temperature as taken by the thermometer did not coincide.

DR. DENHAM.—The paper is so exhaustive that Dr. Macan has left us very little to say. The subject is one of very great practical importance. Milk fever, like toothache, though it often gives a great deal of pain and annoyance, very seldom kills any one. I should have liked that Dr. Macan had given us some information as to the comparative frequency of milk fever in primiparæ, and in cases of subsequent labour. My experience is, that in cases of first children, milk fever is more severe and attended with more constitutional disturbance than in subsequent labours. It is often very difficult to say what the cause of the affection is, but my opinion is that, as a rule, milk fever is more a constitutional disturbance, consequent upon the new secretion about to take place, than anything connected with the formation of the milk itself. According to my experience, the fever precedes rather than follows the formation of the milk secretion. Before much distension of the breasts at all, I have seen severe rigors followed by a marked increase of temperature, and almost invariably an increased quickness of the pulse accompanied by thirst. It is very important to distinguish the rigor of milk fever from that which occurs in other puerperal diseases. The milk rigor is only evidence of the formation of milk, while other rigors are of much more

^a Carpenter’s “Physiology,” 7th ed., p. 923.

^b Archiv. f. Gynaekologie, Vol. VIII., p. 312.

serious significance. My experience is that, when a patient has had during the night a severe rigor accompanied with heated skin and thirst, and when it is found that afterwards the breasts are beginning to distend, there is very little cause for uneasiness. It is important, however, to distinguish between such cases and those in which, with flaccid breasts, there is rigor followed by headache and heated skin. My own experience is that, as a rule, milk fever is comparatively innoxious; but when with the rigor, heated skin, and quick pulse, the breasts are flaccid, there is much more reason for alarm.

DR. HENRY KENNEDY said there were two points of a general character on which he wished to speak. The author had referred to a connexion between milk fever and swelled leg. But as the former very usually occurs within the three first days after delivery, and the latter not till the twentieth, Dr. Kennedy thought there could be no necessary connexion between them—if, indeed, any at all. As to the discrepancy between the temperature and the pulse, to which Dr. Macan had referred, Dr. Kennedy had seen it in other diseases besides that under description. It was common in fever, and no sound conclusion could be arrived at where such contradiction, as it might be called, existed. Some most fatal acute diseases did not cause a high temperature; and on the other hand a very rapid pulse was not, especially in females, necessarily fatal. When a high temperature and a rapid pulse went together, then danger was to be found. There was still another point to which Dr. Kennedy wished to refer, nor was he aware if the author had alluded to it. He spoke of temperament, which all were aware had such a wonderful influence, particularly in females. Some, whilst performing what surely was a healthy function, the formation of milk, exhibited feverish symptoms, often of a very high character. Even menstruation was attended in some by feverish symptoms. Civilised habits had much to say to these varieties, but there could be no doubt temperament also played an important part. These considerations Dr. Kennedy thought might help to explain some of the points so ably advanced by the author.

DR. MACSWINEY.—The research of Dr. Macan has been made very manifest by his elaborate paper. There is one point, however, to which I would wish to call his attention. He has, I am sure, seen a paper which recently emanated from a New York physician, and in which views contrary to those ordinarily entertained on the subject of milk sustained by argument. That a fever often occurs with headache, thirst, fever were broached, while at the same time very heterodox views—I do not say erroneous—as to the general treatment of puerperal women, were and heat of skin, preceded by rigor, after parturition, is universally recognised. On the other hand some parturient women will, after three days or so, present a most completely formed breast of milk without its secretion having been accompanied by trouble or inconvenience of any

kind whatever. Now what is the explanation of that? Dr. Kennedy has thrown some light on it by pointing out that ordinarily the process is a natural one, and that it is but reasonable to expect, *à priori*, that it would be unaccompanied with pain or other disturbance. That seems to be the proper view to take of the matter. Explanations of its occurrence in females in a state of civilisation have been numerous. This state of fever has been ascribed, in an able paper, to a total mistake in the mode of treating parturient women as to regimen, diet, and general management, after delivery. It is stated that just in proportion as the starvation plan of treatment is carried out, will be the trouble, pain, inconvenience, fever, irritation, and natural rebellion of the organism against the proper sustenance required by it. This is the argument which has been broached and ably supported, and I would direct Dr. Macan's attention to it.

THE PRESIDENT.—I have not unfrequently seen breasts overflowing with quantities of milk before parturition took place. Within the last few months I saw the case of an unmarried girl having something like milk fever, which ended in an abscess of the breast, and gave her a great deal of trouble indeed. She was a florid plump girl, and her menses were quite right. She had tension of the breasts, and all the symptoms of a woman who had been recently confined. The great point in such cases is to diagnose whether it is milk fever or something more serious; whether it is an ephemeral attack, or one caused by inflammation of the uterus. I am disposed entirely to agree with what Dr. Denham has said, that if you have milk fever you must have tense breasts; but that if the fever be accompanied with flaccid breasts, there is cause for alarm. I am at this moment rather uneasy about a patient of mine who was confined the day before yesterday, who has fever to-day, with a pulse at 120, and whose breasts are quite slack.

DR. MACAN said, in reply, that what is generally known as milk fever is undoubtedly more common in primiparæ. The explanation of this is easy if we believe the fever to be due to the changes in the breasts which precede and usher in the secretion of milk, which would naturally be more likely to produce disturbance in the system when the glands were, for the first time, called on to perform their functions. He had not investigated whether the fever, due to over-distension of the breasts, was also more common in primiparæ. The question at issue is not whether women are or are not often attacked on the third or fourth day after delivery by a fever which precedes or accompanies the secretion of the milk, and which soon terminates in sweating. The old books are full of such observations, and we have no wish to question their accuracy. The whole point is—what is this fever due to; is it caused by the first secretion of the milk? The older writers did not draw any sharply-defined line between milk fever and puerperal fever. They nearly all say that if the

patient be imprudent or catch cold, or if the milk be driven in on the blood, the milk fever may run into puerperal fever. It has been said that not putting the child to the breast early enough is the cause of milk fever; but Hunter and White said that milk fever was absolutely prevented by *not* drawing the breasts. The great question is—what is the true explanation of the fever? Of late years it has been shown that a number of causes may produce fever which exactly resembles so-called milk fever. Thus, there is the closest resemblance between so-called milk fever and surgical fever, as described by Paget. The wound in the puerperal woman, to which the fever is due, is either rupture of the perinæum or the more general trauma of labour. If so-called milk fever be found to precede the secretion of the milk, it obviously cannot be due to over-distension of the gland. On the other hand, when the breasts are over-distended we have positive evidence of inflammation being present, as evidenced by the redness of the skin and the great tenderness on pressure, and it is therefore not difficult to explain the fever which accompanies this condition. Our president has mentioned a case where inflammation of the breast, in an unmarried woman, produced a fever like milk fever. Why then should not inflammation of the breast produce fever in the puerperal state? Even cracked nipples may, as every one knows, cause an elevation of temperature, and Winckel gives a case in which the thermometer from this cause rose to 105°. The diagnosis of so-called milk fever from puerperal fever is, at the commencement, very difficult. This difficulty is greatly increased at the present day from the common practice of removing the placenta by pressure applied to the fundus. For if the pressure be severe, it may cause subsequent tenderness; and if, along with such tenderness, we get a rigor, it would, I think, be a very hazardous prognosis to say that the patient was not getting puerperal fever, even though the breasts were full of milk. With respect to Dr. Kennedy's observation, I think that since the time of Hunter and White the idea that there is any connexion between milk-leg and milk fever has been entirely exploded. Mental emotion has a powerful influence during the puerperal state, and I mentioned in my paper that Professor Carus gives it a place among the causes of so-called milk fever. It is by no means certain that any elevation of temperature accompanies menstruation, for though Dr. Squire says a rise does occur, Professor Wunderlich says there is none, and I prefer to adopt Wunderlich's opinion. In my paper I did not attempt to go into the treatment of so-called milk fever, but I agree with Hunter, that in the great majority of cases no treatment is necessary. White recommends the breasts not to be drawn, and says if they are very painful we may rub them with warm oil; "very generally, however, nothing is required but patience for a few hours, and the case will always end well." As long ago as 1761, Dr. Astruc, physician to the King of France, spoke of

pressure as being a common method adopted to prevent the secretion of milk, and strapping the breasts is certainly a most excellent way of preventing the secretion. Starvation has been put down as a cause of so-called milk fever, but I myself have had no experience that would enable me to say whether it be so or not.

Improved Ecraseur with Double Action.

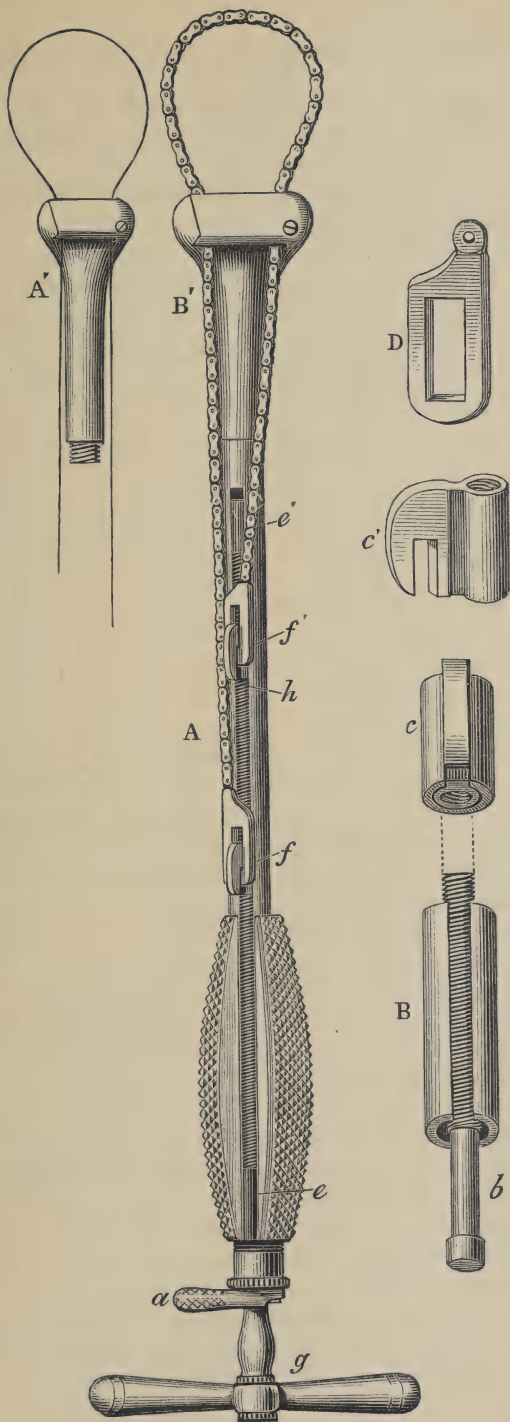
DR. DENHAM, in introducing Mr. Denham, who had a new form of ecraseur to exhibit to the Society, said that in the removal of tumours from the uterus considerable difficulty was experienced in reference to the wire generally used when the tumour was very large. It was frequently found that a large portion of the tumour was left undivided. He (Dr. Denham) had frequently experienced that difficulty, and was sure that Dr. Kidd would bear him out as to the great annoyance the operator experienced when he found a portion of the tumour still uncut. It then became necessary to undo the chain, and re-tighten the entire arrangement. His son, whom he now introduced, although only a student, had invented something which he (Dr. Denham) believed to be worthy of the attention of the Society.

MR. DENHAM.—Mr. Chairman and Gentlemen—My first duty, I feel, is to thank you sincerely for the permission you have granted me in allowing me to address the Society. It would be better before exhibiting my instrument to show what I consider to be the defects in the ecraseur in general use. Supposing you desire to remove a tumour with the ecraseur larger in circumference at its thickest part than the length of the screw of the ecraseur, necessarily, when you have completely worked the wire down to the end of the screw, there is still a piece of tumour to cut through and no more screw wherewith to work. Now you must relax the screw, unfasten the wire from its attachment at the cross bar. This unfastening of the wire, if it be stout, is no easy matter, sometimes the use of a pliers is necessary; having then shortened the wire it must again be fastened to the cross bar, and the operation continued.

The principal defects, in my opinion, are—

1. The great difficulty of shortening the wire *in situ*.
2. The delay occasioned.
3. The danger of hæmorrhage from the movements made in relaxing and tightening the wire, which are often very great.

In the accompanying plate the instrument is represented as half size, the minutiae accompanying full size. At either end of the screw *e' e* there is a groove into which the two hooks *f' f* exactly fit; these grooves are simply part of the screw without a thread, as represented in *B'*, *b* being the groove. In order better to make myself understood, I shall go through the *modus operandi* of using this instrument.



Having it adjusted with a chain, in the manner here represented, each end of the chain having a steel loop attached, D represents the loop; this can be hooked and unhooked at pleasure. The upper hook f' resting in the groove e' , the lower hook f resting quite close, but on the screw. When the handle g is turned f travels down the screw, f' remaining motionless in the groove; having worked f down to the end of the screw until it slips into the groove e and remains motionless; then a is pulled over, g is pushed up into the space left by a , then the end of the screw strikes f' , on turning the handle g , f' becomes wormed on to the screw off the groove; f' then travels down the screw after f , and stops close to f' . If wire be desired to be used all that is necessary is to remove B' and substitute A'.

The advantages which I consider this instrument to have are—

1. Simplicity in construction and application.
2. Having two separate instruments combined in one, that is, being able to use both wire and chain on the one instrument.
3. The ease with which it can be worked.
4. The rapidity of its action.

Finally, I may state that this instrument is capable of removing a tumour 14 inches in circumference without making any change in arrangement.

CHAIRMAN.—I congratulate the Society upon the fact that the son of one of its most distinguished members should have thus early evinced a desire to tread in the footsteps of his father, and employ his talents in devising a means for facilitating and simplifying one of the most critical surgical operations in obstetric practice. Mr. Denham seems to have accomplished his object very effectually.

DR. KIDD.—I think the instrument submitted to us is a very great improvement. I have frequently had to stop the performance of an operation for the removal of large intra uterine tumours, to tighten my wire, and that is in itself a difficult process which complicates the operations very seriously, and is always a source of very great trouble. This instrument will enable us to do that with great facility, without retarding the operation more than a few seconds, and, as far as I can judge of it, its action ought to be perfect. It will enable us, in removing very large tumours, to use a sawing motion. The instrument is a most valuable one, and I believe it will prove a great benefit to the operating surgeon.

DR. MORE MADDEN.—I had an opportunity of testing the instrument the other day, and I believe it will confer a great practical benefit on operative gynaecology. There is no doubt that Mr. Denham has improved the ecraseur, and made it a more handy useful instrument than it was, and I only rise now to suggest that it would be a very desirable thing if the instrument could be made at such a price as would render it more

accessible than it at present is. As now made by White, it is a very expensive instrument.

MR. DENHAM.—The instrument cost me £5. But if you consider that it combines the two instruments in ordinary use, you will see that it is not so expensive. The ordinary ecraseur costs about 50s., and the chain ecraseur costs 50s. more. Most obstetricians have one of each, but I believe the instrument could be made for considerably less.

The Society then adjourned.

RATIONAL TREATMENT OF LEAD POISONING.

IN acute lead poisoning the aim of the physician is first to render the lead in the alimentary canal insoluble, in order to prevent its absorption, and then to remove the (relatively) insoluble substance from the system. Both of these indications are very well met by the administration of sulphate of magnesium, which first renders the lead salt insoluble, and then carries it off by the bowels. The chronic form of lead poisoning is due to the absorption of lead into the tissues and organs, and differs from acute poisoning mainly in the fact that while in the acute form of the disease the great bulk of the poison is really outside of the body and producing in the various parts of the alimentary canal its own peculiar species of irritation, modified to some extent by the action of the smaller portion which is absorbed, in the chronic form nearly all the poison is inside of the body and producing its own peculiar physiological effects in the deterioration of the tissues and organs. This difference in the locality of the poison ought at once to indicate rationally a different mode of treatment, and, to some extent, it is generally believed that this is attempted in the usual treatment of chronic lead poisoning—viz., by the administration of iodide of potassium. Dr. George Hay, however, on rational, rather than on experimental, grounds, says that instead of iodides we should exhibit chlorides. In chronic lead poisoning, he says, the lead is eliminated principally by the kidneys in the form of chloride of lead, or of oxide dissolved by chlorides or other alkaline salts. That is to say, nature eliminates the lead by making it soluble in water, in the form of chloride of lead, and then removing it by the urine. Therefore, when we give the patient iodide of potassium we simply render the lead less soluble than nature would make it, and instead of hastening its elimination we retain the poison in the body. Dr. Hay suggests, that of all the chlorides, that which seems to be most suitable to the economy is the common chloride of sodium. It might be administered in cases of chronic lead poisoning in doses of one drachm three times a day, more or less, but never in quantity to occasion nausea.—*Phil. Med. Times*, Mar. 16.

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

SESSION 1877-78.

President—G. F. WALES, M.D.; F.R.C.S., Ed.

Hon. Secretary—WILLIAM WHITLA, M.D.

Tuesday, January 15th, 1878.

The President, DR. G. F. WALES, in the chair.

The late Dr. Stokes.

PROFESSOR DILL moved, in a suitable speech, and DR. WHEELER seconded, the following resolution, which was passed unanimously:—

“That it is with feelings of unfeigned sorrow we have heard of, and are this day called upon to record, the death of Doctor William Stokes, of Dublin, Honorary Member of this Society, whose great loss will be long and deeply felt, not only by every member but by the profession at large, and by the whole community, as that of a man of whom it may be justly said, and in the truest sense of the words, ‘that he was a perfect gentleman, an accomplished scholar, a great teacher, and an able physician.’”

Specimen of Jaundiced Liver and Gall Bladder.

DR. WHITLA showed the liver and gall bladder of a patient, aged seventy, who had died of jaundice, and gave a brief history of the case. The jaundice had lasted, as far as could be determined, three months and a half; the patient was insane; and, when first coming under observation, a large tumour was detected in the right hypochondriac and epigastric regions. The question of its being an accumulation of fæces or a malignant growth was weighed, and it was determined to try the effects of a series of enemata. To this the patient rigorously objected, and it was found impossible to inject anything into the colon. Laxatives and ol. ricini were ordered, but she refused all medicines; once a little jalap was administered in wine; the patient detected it, and could never be again induced to take it. Three weeks before death diarrhœa set in—the evacuations resembling fluid tar—and she gradually sank. On opening the abdomen, the colon was found filled with hard scybalæ—a large accumulation being found at its right bend; its walls were thickened and opaque, a low form of inflammation having extended through its coats. The gall bladder was full; the cystic duct was almost closed, and near its junction with the hepatic was much narrowed; the lower part of the choledoch duct was

normal, a large mass in the colon having by its pressure almost entirely closed the biliary canal near the junction of the cystic and hepatic ducts to form the ductus communis choledochus; a *trace* of bile was found in the small intestines. The other organs were healthy, except the kidneys, which were congested, but otherwise unaltered.

DR. SPEER remarked that the structure of the liver was apparently natural, and asked had it been examined by the microscope.

DR. GRIBBON had seen several cases where impaction of fæces had produced diarrhœa. He, however, had generally noticed that some scybalæ had come away in the motions, the nature of which he often found resembled those described in the case before them.

DR. DILL objected to the term diarrhœa being used in a case where there was impaction of the fæces. The word *dysentery* should be used when describing a case of this sort, especially where it was evident that there were very considerable quantities of blood in the motions. He had himself found ipecacuanha useful, and believed, had it been tried here, good results would have followed.

DR. WALES thought the specimen very interesting. As regarded the symptoms during life, the presence of jaundice and diarrhœa were especially liable to mislead. He thought that the exhaustive discharge from the bowels in one so advanced in years was quite sufficient of itself to cause death. Probably, as had been said, death was owing to a variety of causes.

Specimen of Uterus and Ovary.

DR. WHITLA exhibited the uterus and ovary removed from a patient after death. He thought of showing the specimen, which he obtained the day before, not because there was anything remarkable in the cystic degeneration which was present in the ovary, but he thought it interesting as Dr. Cuming was about to relate to them a case of rupture of the ovary which had caused death in a young patient. The cyst before them was not larger than a pigeon's egg; it was a purple colour, owing to the flattening out of one or two veins which occupied its surface; the contents were clear and transparent; the walls were of such tenuity that apparently a very slight blow over the abdomen would cause its rupture. It gave rise to no symptoms during life. The uterus to which it was attached presented some very unusual features. It had studded in its walls about a dozen small fibroid tumours; two near the fundus were sub-peritoneal; this accounted for the shotty feel of the organ. The tumours were hard, about the size of large hemp-seed (one was larger than a cherry-stone), and were distinctly encapsuled, shooting out of their bed upon a slight incision being made over them. From the roof of the uterus hung a small polypus into its cavity. The uterus was removed from an old virgin of seventy-four years.

DR. WHEELER thought the length of the neck of the uterus was remarkable. The organ would not weigh two ounces, and it seemed to be all made up of cervix. With the tumours, which he thought were fibromuscular, he would have expected some hypertrophy instead of atrophy.

DR. DILL thought the uterus, considering its source, was normal in size and weight, and was not more atrophied than the age of the patient would lead him to expect. While recognising the polypoid nature of the small internal tumour, he would be suspicious of the schirrous character of the isolated growths through the uterus; but, not having examined them microscopically, he could not speak positively. He would suggest their being carefully examined, and perhaps cancer cells would be found.

Fatty Degeneration of the Liver.

DR. WHITLA showed a section of a liver, removed the previous day. It presented, in a well-marked degree, all the characters of the fatty degeneration. The cells were broken down, and filled with granules and oil globules, the field being obscured with *débris* of cells in all stages of transformation, from normal to oil globules. There being some interesting points in the history of the case, he would ask Dr. Wheeler, under whose care the patient had been, to mention them. The entire organ would weigh, he thought, between 6 or 7 pounds.

DR. WHEELER said the case was to him a very interesting one, especially as regarded the symptoms—viz., violent and agonising attacks resembling angina pectoris; the pain being referred first to the region of the heart, and afterwards over the abdomen in its upper part. These attacks became more and more frequent, and the patient sank in a typhoid condition. The enlarged liver could be felt during life, but it was not clear whether its enlargement was owing to one of the degenerations or to cancer. A large tumour had been ligatured in the rectum about two years previously, the nature of which at the time he believed was hæmorrhoidal or polypoid, though a distinguished surgeon who saw it thought it was malignant. This always threw a doubt in his mind when thinking over the probable nature of the tumour in the hypochondriac region. In the gall bladder was found a large gall stone, which he thought caused the attacks of pain. The examination was made under difficulties; and he regretted the heart was not examined, the chest being unopened. The patient was never jaundiced. All the abdominal organs were healthy.

DR. CUMING then related a case of rupture of the ovary in a young woman, followed by death. The history and description will appear in a future report.

Tuesday, the 29th January, 1878.

The President, DR. G. F. WALES, in the Chair.

Case of Psoriasis.

DR. HARKIN gave the details of a patient's case who had suffered for nine years from psoriasis. He was a boy aged now about fourteen. He had been so completely covered with the eruption that he was unable to appear in school or be sent to any business. He narrated the various remedies which he had tried—arsenic, phosphorus, iron, &c., as well as nearly every known form of ointment. At the conclusion of each line of treatment he was found to be in much the same state as when he commenced. Hearing of chrysophanic acid, he determined giving it a trial. In three weeks after its use the skin was healthy, and no trace of the eruption was visible. The results were so remarkable that he thought it his duty to produce the patient before the Society. Dr. Harkin then showed the boy, whom he had not examined for several months—*i.e.*, since the cure by the acid—and it was apparent that there was some slight return of the disease, as a few spots were visible on the back of the chest, the skin elsewhere being entirely healthy.

DR. WHITLA (Hon. Secretary) showed a patient who had been under his care for three months, and taken large doses of arsenic for a very extensive eruption of psoriasis, some of the patches on the body being as large as the palm of a man's hand. The arsenic had no effect whatever upon the eruption, which continued to increase. Hearing from Dr. Harkin of his success, he commenced to try the chrysophanic acid, exactly seven days ago; and in order to satisfy himself about the result, the ointment was very carefully applied to one side of the body only. At no place had the application on the trunk extended across the middle line. The patient, a boy aged thirteen, then stripped, and a remarkable difference was observed between the two sides of the body resulting from the seven days' treatment. In no place was the eruption entirely removed, but it was evidently fast disappearing. He could not satisfy himself that mere friction with plain lard could produce any such marked effect, especially as tar ointment had been carefully applied before. He merely produced the patient to show the members the remedy in process of application, by way of illustrating Dr. Harkin's case. Attention was then drawn to the most unusual expression of the patient from the colouration of the skin round the eyes, which gave him the appearance of an owl; and he thought that this effect must be produced by the acid after its absorption. The bed-linen and clothes of the patient were destroyed by its staining properties.

PROFESSOR DILL thought the cases highly interesting to the profession. He had looked upon psoriasis as an opprobrium to the healing art, but the

cases which he had seen before the Society encouraged him greatly, and if the cure remained permanent, he would say it was a great triumph. He had formerly believed it was a blood disease, but in those cases the local remedies seemed to have the most effect. Dr. Harkin's case, though after several months showing a tendency to return, he thought, was most remarkable. Did the acid act locally or through the blood? This, which he thought a most interesting point, seemed to be answered by Dr. Whitla's case, where there was a decided change on that side of the body to which it had been applied.

DR. M'CONNELL was not a believer in the chrysophanic acid. As far as he could see, he thought that psoriasis could be temporarily cured by almost any application, but invariably returned. He had produced results quite as satisfactory as in the cases shown, by a simple lotion covered with oil silk. In his own experience he had not met with any successes; in all the disease returned. He had no experience of the acid, but would try it in a case of twenty-five years standing, which he had under his care.

DR. ESLER was disappointed in Dr. Harkin giving no account of the history of the remedy. He detailed some particulars about its early introduction into Belfast. He agreed with Dr. M'Connell, that nearly every application could be made to remove the scales, but the disease always returned. Hebra said psoriasis was incurable; if so, there was little use in chrysophanic acid.

DR. SPEDDING had much experience of the disease in the dispensary, but found it always returned. He thought more stress should be laid upon the constitutional treatment, as he believed it occurred generally in strumous constitutions. He found much benefit from cod liver oil and iron.

DR. DEMPSEY thought the cases, upon the whole, very satisfactory, but thought Dr. Harkin's was showing signs of return.

THE PRESIDENT, in reviewing the different theories about the pathology of psoriasis, and the alterations which knowledge of the disease had made in its treatment, was inclined to think most favourably about the success of chrysophanic acid treatment. He commented upon the published cases of Mr. Squire, to whom he thought great credit was owing. He hoped to see the cases brought before the Society again, at the expiration of a few months.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

President—EDWARD HAMILTON, M.D.

Secretary—E. H. BENNETT, M.D.

Ascites with Hydronephrosis of the Kidney.—DR. LYONS said: The specimens I exhibit belonged to a man of about fifty years of age, who was unfortunately addicted to drinking in great excess. He had the morbid thirst for drink to such a degree that it seemed to be entirely beyond his control. He was admitted into the Whitworth Hospital between two and three years ago, and was then extensively dropsical. Both his lower extremities were so to a large degree; his face was puffed, and his abdomen distended, but not remarkably so. His urine was abundant; it was loaded with albumen, and its specific gravity, though varying from time to time, never went below 1015, and was occasionally higher. He was threatened with gangrenous destruction of the integuments of the lower extremities, from the enormous amount of distension. I punctured his legs, and thus gave exit to a very large quantity of fluid, which afforded, for the time, very considerable relief. The tendency to accumulation of fluid in the legs seemed to be replaced by a tendency to the accumulation of fluid in the abdomen; and the distress he suffered from fluid in the abdomen and the distension of his colon was so extreme that, having witnessed the good effects of tapping in two other cases in the ward, he entreated to be tapped. I accordingly tapped him, and drew from his abdomen about nine quarts of fluid; but although this man's sufferings from the distension of his abdomen were as extreme as ever I have seen in any one, yet the quantity of fluid drawn off from him never at any time reached much beyond nine or ten quarts, which all familiar with the operation of tapping know is, on the whole, a moderate amount. The fluid withdrawn was so very peculiar that I requested Professor Tichborne to examine it. It was of a milky-white colour, and, in a glass vessel at a distance on a table, any one would have supposed it was milk. Dr. Tichborne examined the fluid, and was kind enough, on the 15th December, 1876, to send me the following report:—

“Composition of ascitic fluid:

Ash	-	-	-	-	0.67
Organic matter	-	-	-	-	1.3
					<hr/>
Total solids	-	-	-	-	1.97 per cent.

“This solid matter contained, as regards the mineral constituents,

chlovides and phosphates of sodium; albumen, 1·01 per cent.; and urea (or a similar body), 0·2 per cent. Specific gravity of fluid=1011."

He puts an asterisk at the word "urea," and says, in a note, "urea, or a similar body." The history of the case from that time afterwards may be briefly summed up. The fluid continued to accumulate in the abdomen, so that, at the end of every three weeks, he prayed and besought to be tapped. He had the greatest possible faith in the operation of tapping, he got such immediate and direct relief from it, and was very urgent about it when the fluid reached a certain quantity. About ten quarts of fluid seemed to give him great distress. Altogether he was tapped eleven times, ten by myself, and once, in my absence, by Dr. Harvey. The fluid at all times had precisely the same peculiar milky colour and character, and was I presume, of the same specific gravity and composition as that analysed by Dr. Tichborne. A period now arrived in this remarkable case in which the dropsical effusion seemed to be at an end; and, after eleven tapplings, it was never necessary to tap him again, nor, strange to say, did he require the drawing off of any fluid from his lower extremities. He got, in fact, into such a tolerably good condition of health that, at his own request, he left the hospital, and resumed his work for a considerable time. But his unfortunate propensity again mastered him; and, on more than two subsequent occasions he had again to seek admission into the hospital, not so much from any reaccumulation of dropsical fluid in the abdomen or lower extremities as from the state of exhaustion into which he had reduced himself by excessive drinking. He drank everything that he could lay his hands on—whiskey, ale, porter, brandy, and wine. And being one of those cheerful good fellows he seemed to have an unlimited command at all times of the good (or bad?) things of life in the way of drink, and was supplied by his friends freely on all occasions. While away from hospital I saw him repeatedly in the streets going about. He had been on one occasion a pedlar, then he took to labouring work, and he was finally occupied in some duties connected with the Midland Great Western Railway. He was admitted to hospital for the last time some three weeks since. He admitted that he had had a heavy bout of drinking, and it seemed to have finally floored him. On the previous occasions he had exhibited the greatest confidence in his prospects of recovery, but now he seemed to have lost all hope, for he said that he had only come in to die. A certain amount of dropsical effusion had taken place in the lower extremities, but it was not very remarkable considering what was present on former occasions. But he now exhibited what had never appeared on previous occasions—namely, a very great amount of urgent dyspnœa; and, if I can attribute his death immediately to any one complication more than another, I think it was due to the thoracic complication. He died the day before yesterday. The *post mortem* examination was very carefully made by Dr. Harvey. We found first that the liver

was certainly not in a state of cirrhosis. I cannot agree with those who think that whiskey or even porter drinking is by any means a necessary antecedent in the history of cirrhosis of the liver. In some of the most remarkable cases that I have ever seen the individuals had been temperate all their lives, and one case was that of a lady who had never at any time of her life tasted any kind of ale or fermented liquor, and she suffered in a most marked degree from cirrhosis of the liver, which was the cause of her death. My opinion is that the alcoholic liver is one of a widely different kind, and I think that in the future transactions of the Society we may, perhaps, be able to clear up a good deal of error with regard to the connexion between cirrhosis of the liver and the use or abuse of alcohol. I do not think any one can say that in the present case the liver is in a condition at all approaching to or like that of cirrhosis. The capsule of the organ is no doubt thickened here and there, but I would myself be disposed to attribute this to a certain amount of subacute peritonitis, which is pretty generally the result of the operation of tapping. The liver is of a good brown colour, but is somewhat smaller than it should be. The spleen exhibits a good deal of inflammation of the capsule here and there, but still is small in size, and not at all the spleen that we are familiar with in connexion with cirrhosis of the liver. I should observe that we are very much indebted to Dr. Harvey for the examination which he made in this case, as well as for his able assistance on all occasions. On opening the abdomen the appearances were striking. The two kidneys were united by a band of tissue, constituting a more or less complete example of horse-shoe kidney. Whether the band of connecting tissue contains any renal element or not we are not in a position to say. However, Dr. Harvey will be able to report on a future day whether the band be renal or ligamentous. The right kidney presents a marked amount of fatty degeneration. The capsule is somewhat adherent, but tore pretty readily without taking away much of the renal tissue. You will observe even in the exterior of the organ a considerable tendency to lobulation, which is evidenced still further by the sulci being deeper than usual. The cortical portion of the kidney is in an advanced stage of fatty degeneration; we have not yet been able to make a full examination of it under the microscope. The pyramids also are in a state of fatty degeneration. The principal interest is in connexion with the left kidney, which shows an extreme amount of hydronephrosis—the kidney being separated into two distinct portions, and the posterior and the superior being entirely apart. In this kidney, also, the tendency to lobulation is pretty well marked; and it seemed as if a congenital condition of lobulation had been present in both kidneys. There is a considerable amount of urinous fluid retained in the kidney; and, having regard to Dr. Tichborne's analysis, so kindly made on a former occasion, and showing that the elements in the contents of the abdomen then

submitted to him were either urea or a similar body, it was now acutely suggested by Dr. Harvey that the escape of urea from the kidneys, by transudation, into the abdomen would account for the presence of the urea there. It is, therefore, extremely probable that the body found was urea, and that it escaped by exudation from the secretion of the kidneys into the abdomen. I do not think we could at all say that the total amount of fluid in the cavity of the abdomen at any time was attributable to escape from the kidneys in this way. There must have been some other cause for the collection of fluid in the abdomen; but what it was I do not at present see. We know that it is not a necessary result of renal disease that there should be an accumulation of fluid in the cavity of the abdomen, although it may be so with respect to other organs. The abdomen is certainly not the favourite seat for the escape of fluid in large quantities in the case of renal disease. Where other organs are surcharged with fluid, we often find that effusion takes place at the later stages into the cavity of the abdomen; but here, for two years at least in the history of the case, there had been effusion to a very large amount into the cavity of the abdomen. Following down the ureters, Dr. Harvey noticed that one of the renal veins lay over the ureter in such a position that when the ureter was distended with fluid the vein was subjected to a very considerable amount of pressure from beneath, so that the circulation through the kidney was no doubt considerably impeded. The ureters are pervious throughout, and without any obstruction; and the catheter passed easily into the bladder in both instances—in that of the left kidney particularly. The passage is quite free to the right kidney also, but that there is some obstruction internally is evident from the fact that though the fluid in the bladder drained away this morning, there has been since a gradual escape of renal fluid from the enlarged calices and pelvis of the kidney. Whether during life the pressure of the vein on the ureter was sufficient, during the advanced stages of the disease, to retain this mass of fluid, and perhaps even more in the enlarged sac then formed by the calices and the pelvis, is a nice point to determine. What the immediate origin of the hydronephrosis in the case may be I do not at this moment see. As I have already stated, I believe that death was the result mainly of the thoracic complication. Dr. Harvey found the right lung in a condition of extreme engorgement, with evidences of old pleuritic inflammation, and adhesions to the parietes. The upper half of the left lung is a good deal engorged also, and the lower portion of it affords a beautiful and perfect example of emphysema. There was emphysema in the greater portion of it, but the lower portion manifested true emphysema, as was evidenced by the blowing up and distension of the pleural tissue and the escape of air. When the left kidney shall have been opened, and Dr. Harvey enabled to make a full examination, we shall be better able to say on a future

day whether there is anything to account for this peculiar hydronephrosis. Whether it was partly of congenital origin or was due to mechanical obstruction is not very apparent at this moment. The heart was tolerably healthy; it was a little enlarged, but there was no valvular lesion of any kind.—*December 1, 1877.*

Excision of the Hip.—Dr. BENNETT said: These specimens exhibit the result of the operation of excision of the hip, practised as a treatment for morbus coxæ. Unfortunately the case is, in one sense, too complete, for we have both the portion of the femur removed by the operation, and also the innominate bone and remainder of the femur, removed from the dead body many months afterwards. It was one of ordinary morbus coxæ in a child of between nine and ten years old, in whom the disease had reached the stage of suppuration. He was admitted into the hospital on the 16th of October, 1876, with morbus coxæ of some standing, a lengthened leg, and an everted foot. On the 13th of March following there appeared, without pain or any marked symptom attending it, a large abscess in the trochanteric region of the joint (by this time the limb had become inverted, and apparently shortened). This pointed just below the attachment of the tensor vaginæ femoris. He suffered from well-marked hectic. The temperature chart which I produce covers the entire period of his illness. The hectic was established at a temperature of 101° , with an evening elevation of 101.5° , and a fall to nearly a normal temperature in the morning. This condition of affairs extended for a considerable time, and the boy suffered from sweating and diarrhœa. The abscess pointed, and we opened it with antiseptic precautions, and the result was a most pronounced fall of temperature for some days. Then there suddenly came on a great rise, which was coincident with a development of the phenomenon of putrescence in the dressing. All through the child suffered from a diarrhœa which we could with difficulty control; and there was consequently great difficulty in keeping the dressings clean. They became soiled, and the antiseptic treatment failed from accidental soiling by fæcal matters, under the care of a nurse imperfectly trained in the treatment. The high elevation of temperature continued. The case went on with extreme hectic until the 23rd of the following month of April, and the condition of affairs was such that death became imminent, as the boy was wasting rapidly. In fact, he was then so low, from the prolonged irritation, that some of my colleagues thought it would be rash to attempt any operation. Just at this time Mr. Spence, of Edinburgh, saw the boy with me, and strongly urged excision, even in spite of the condition of affairs. I accordingly operated. There was nothing about the operation requiring to be noticed except this—we had a very free discharge of pus from an abscess which opened on the outside; and it

was clear at the same time that a large collection of matter existed on the inner side of the thigh extending half way down the femur. It was a nice point to determine whether an abscess passing down along the femur was to be taken as an indication of disease of the shaft of the femur; and to a considerable extent I made up my mind that there was no positive evidence of disease of the bone. No enlargement of the bone could be felt. I inferred that the collection of matter was the result of a want of drainage from the external opening. When we made the necessary incisions for excision there was a great discharge of matter. I do not think there was any other feature in the operation. The section was made through the base of the neck of the bone, and through the great trochanter, and the detached trochanter with the muscular attachment were perfectly healthy. At only one point below this level in the femur did we strike on diseased bone, where there was a species of softening; this was on the shaft at the upper end of the inner side of the shaft. On passing my finger into the acetabulum I found that it was bare and rough, and that considerable roughness existed on the inner side, but there was no very great area of the bone exposed. So small was the area of disease outside the limits of the acetabulum that I considered it unnecessary to attempt to remove the floor of the acetabulum. A marked fall of temperature followed the operation for a fortnight, and the morning and evening changes of the hectic ceased. From that time out the case again went backwards, but so slowly that at some times we had hopes that the final issue would be successful. The cause of the hectic can be seen in the specimens. We have the parts almost absolutely as they were in the operation. The front of the capsule was open. I had avoided cutting against the bridge of the capsule in front in turning out the bone, so that I was able to save the ligamentous connexion in front. There was a tolerably free passage for purulent matter, but still the caries seems to have progressed. The disease of the ilium has been prolonged upwards and outwards. Turning inside we find that though the acetabulum was not open at the bottom—that is, that there was no absolute opening that the finger could detect—still a cribriform series of holes had opened into the intrapelvic subperiosteal space, from which a narrow cleft from the joint led upwards and outwards. We had an abscess developed in the iliac fossa, and caries developed on the outside of the ala ilii. This was the cause of the continuous hectic. The upper end of the femur is absolutely smooth, and there is an investment of lymph over the section; it is absolutely sound, except at a minute point where the apex of the bone struck against the diseased ala ilii. There is no disease except where it has been imparted to it by friction against the carious surface of the ilium. If the caries had confined itself to the limits of the acetabulum, and not extended upwards, in all probability we would have succeeded in the

case. The specimen is of interest as showing how the operation most commonly fails in such cases, through the extension of the disease to the pelvic bones in spite of the removal of the head of the femur. The operation was performed in the month of April, and the boy lived until October. His temperature after the operation never reached such a height as it had attained before it.—*December 8, 1877.*

Polypus of the Rectum.—DR. BENNETT said: The specimen I now present to the Society was originally of such small dimensions that it was hardly worth bringing it forward. One half of it is undergoing preparation for microscopic examination. I have brought sections made in a perfectly recent state from the second, of which one is beneath the microscope. The case is one of a disease not very common in this or any country, although all the authors on the special organ describe it—namely, polypus of the rectum. The circumstances under which the case occurred were the following:—A boy, about ten years old, apparently in ruddy health, was brought to me about two weeks ago. His mother reminded me that I had seen him that time twelve months for an affection of the bowel, involving hæmorrhage. I kept no note of the case at the time, but I recollect the facts. He was brought into the out-room, and his mother stated that he had passed blood in some quantity during two or three days. I made an examination of his rectum, but could detect no disease whatever. Hæmorrhage from the rectum in a child of his age being so rare from any other cause except polypus, I assumed at the time that polypus existed, but could find no proof of it. I administered injections, and made examinations both digital and specular, but could obtain nothing except negative evidence. This year he was brought back to me in exactly the same condition, and his mother stated that on and off during the whole year hæmorrhage had occurred. One point worthy of notice was that although these hæmorrhages, according to his mother's account, had been so constant and severe, the child was not in the least degree blanched. He was ruddy and healthy, and had good red lips; and evidently the hæmorrhages, although tolerably frequent, must have been very slight at any time. On examining him digitally, I could obtain no evidence whatever of the disease. I administered an enema, and immediately after the action of it a tumour presented itself in the anus. The boy told me that always of late when his bowels acted with anything like a freedom approaching diarrhœa, the tumour came down, and he had to press it back with his fingers. When examining the tumour we had to be pretty quick, for there was a tendency to the withdrawal of the tumour immediately on the child being put lying down. It presented the usual appearance of a small pile, and might be readily taken for it. The surface was slightly velvety, and of a purplish colour; it was about the size of a hazel nut, and, when

touched on the surface, had a slight tendency to bleed. A probe passed round it showed that it was not anywhere connected with the margin of the anus. Having satisfied myself of its existence, I returned it into the bowel, and waited another day to secure it. After returning it, I passed my finger into the rectum, but could not feel the tumour at all. Next day, by repeating the enema, I got it down again. Having got it down, I withdrew it in the grasp of an ordinary pile forceps, and then learned that it was attached by a pedicle certainly not thicker than an ordinary goose-quill, and almost perfectly transparent. Knowing the tumour to be of rare form, I was anxious to obtain it as little damaged by removal as possible; I therefore handled it with great lightness. I was anxious to put a double ligature round the base, as in some recorded cases hæmorrhage has occurred from the slipping of the ligature. I set to work with an ordinary needle armed with a double ligature, and proceeded to transfix the pedicle. As I did so, accidentally the point of the needle touched the skin, and, the child withdrawing, the polypus broke away. It was lucky I had it transfixed, otherwise it would not have been easy to prevent the receding of the pedicle into the anus; I took it with an ordinary artery forceps, and tied it gently. It was necessary to do so very gently and softly, because it was clear that if we used force the ligature would cut it. Curling is the only English writer who describes exactly the nature of the pedicle which contains a number of blood vessels, so that you must ligature it with the greatest lightness in order to avoid cutting through them. The size of this one, and the character of the attachments, &c., all agree with the description given by Curling, who took his description from the original description given by Lebert. There does not seem to be anything constant about the position of the attachment; it is usually towards the posterior side of the rectum, but there is great variety in the height of the bowel to which the pedicle is attached. In this case, as soon as the tumour receded, the ligature and pedicle withdrew into the intestine, and a great length of string was withdrawn into the bowel. Taking hold of the string with the forceps, I passed my finger into the bowel along it, but could, by gentle traction, barely reach the knot, as the attachment to the pedicle was much higher, and the ligature passed beyond the reach of my finger in the bowel. This explains why we were not able to touch it in its ordinary condition. Many points of interest demonstrating the rarity of the disease appear in the accounts given of the structure. Lebert describes very briefly the details of the tumour, giving characteristics exactly agreeing with what I have given, excepting that the symptoms in the case I have detailed were milder than they generally are, there being no violent diarrhœa or hæmorrhage. In all essential respects, however, the phenomena agreed with those given in the recorded cases. Lebert states that a walnut is the maximum size, the usual size being that of a hazel nut. With reference

to the structure of these tumours, he says that the essential element in these polypi is a considerable agglomeration of follicles, very prolonged, and presenting cylindrical epithelium very distinct along the entire wall. Allingham, in the second edition of his work, says that the microscopic structure of these polypi varies. Generally they consist of a mass of fibro-nucleated tissue, overlaid with blood vessels, and wholly covered, though scantily, with scaly epithelium. Lebert states that the epithelium is cylindrical. The surface of the tumour—which Lebert does not describe, though mentioning it in connexion with the wall of the rectum—is in this case covered with epithelium of a form which is a modification of the cylindrical. I do not know a more remarkable specimen of a glandular tumour than this is. The whole mass of the tumour seems made up of follicles. Between the tubes of the structure, and directly supporting the epithelia, there is fine, soft, connected tissue, exactly the same as the fine tissue occurring in polypus of the nose. The ducts run every way.—
December 22, 1877.

A NEW GLYCOSURIC AGENT.

EVERY one knows the uncertainty of Trommer's test and the bother of Fehling's solution. Dr. Piffard, of New York, states (*N. Y. Med. Record*, 23rd March) that the following will be found a convenient substitute:—

Take of

Sulphate of copper (chem. pure)	-	-	1 part.
Crystallised tartrate of soda and potassa	-	-	5 parts.
Sodic hydrate (chem. pure)	-	-	2 „

Mix thoroughly in a mortar; the more labour spent on this, the better the product. The result will be a pasty mass, which can be transferred to a wide-mouthed bottle and kept till wanted. To use it, take of the mass a piece about the size of a pill, put it in a test-tube, and add about two fluid drachms of water; boil till the mass is dissolved, and the solution has a uniform pale and rather dirty blue colour; then add two or three drops of the suspected urine, and boil again for a moment. If sugar be present, the usual reaction will be manifest. The mass may conveniently be made into pills of proper size, one pill being sufficient for a test.

CLINICAL RECORDS.

Two Cases of Deformed Union, sequent to Fractures of the Leg, necessitating in each Re-breaking of "both Bones." From the Surgical Practice of Mercer's Hospital. Under the care of MR. E. STAMER O'GRADY, A.B., Surgeon to the Hospital, and Member Surgical Court of Examiners, Royal College of Surgeons in Ireland.

THE occurrence of faulty union, so aggravated in degree as to require re-breaking of an already firmly repaired fracture, is of extreme infrequency; the bearings of such will always be matters of exceptional interest to the practical surgeon. The two children whose cases are the subjects of the present "clinical record" were under treatment during the past summer, and within a short period of one another. The sequences of the operation in each of them stood in marked contrast. In the first every result that could be desired was rapidly attained; in the second the slow and tedious attempts at repair whilst the infant was sickening with and openly suffering from an independent malady were in striking contrast to the rapidity with which, a few months previously, in the same individual, firm union (albeit in a deformed position) had taken place after the accident.

CASE I.—*Extreme Angular Deformity accompanying Union after Fracture of both Bones of the Leg; section of tendo Achillis; Partial Division of the Bones with the Chisel, and completion of the Re-fracture with the Clamp and Mallet; perfect result—vide Figs. 1 and 2 on Lithographic Plate.*—Mary M. (No. 2,540), seven years of age, was admitted for the deformity of her left leg, indicated by Fig. No. 1 on the lithographic plate, which renders any lengthened written description of the condition of the limb unnecessary. The degree of the angularity in the bone was greater than what the appearance of the limb conveyed to the eye (or than what would be gathered from the plate), inasmuch as the soft coverings toned down the outline of its prominence. The accident resulting in this state of affairs had occurred two and a half years previously in a remote country district. After it, no treatment whatsoever had been adopted; the little sufferer being merely permitted to lie at rest in bed for some weeks, and the case left to nature. The result was an exceptionally firm consolidation in the position indicated, the angle of union being not far from a right one. There was a considerable amount of exuberant callus, and the integument covering the prominent bone was tense, red, and tender; from its condition it was deemed that it would be inexpedient

FIG. 1.

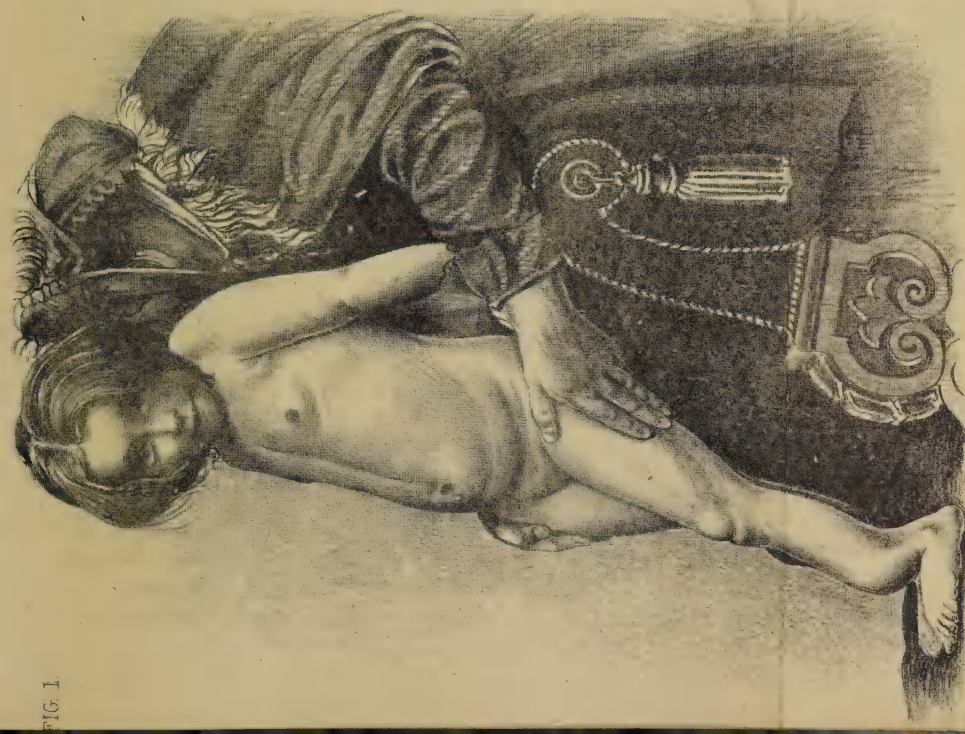
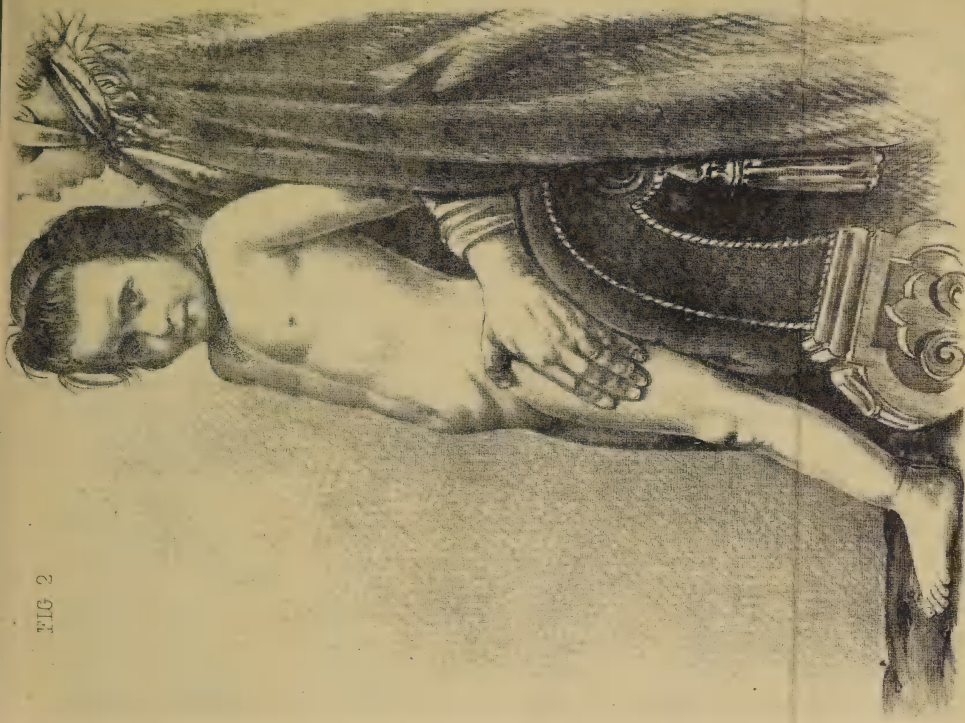


FIG. 2.



to make any firm or sustained pressure here. The tendo Achillis, curved and tense, was thrown into strong outline. This structure having been first divided in the ordinary manner with a fine tenotome, and the little wound closed with adhesive plaster, an incision, five-eighths of an inch long, running in the direction of the axis of the limb, was made on the inner side of the leg over the deformed bone, cutting clear down on its surface. Into this wound a chisel (the edge of which corresponded to it in size) was inserted, and having been partly rotated, was driven on by a few taps of the mallet, so as nearly to perforate the bone. The instrument, after being partially withdrawn, was placed with its edge directed alternately downwards and forwards, then upwards and backwards, so as to divide the bone in the oblique direction indicated. This was all done through the single original skin wound. The limb was then placed in a powerful iron screw clamp,^a suitably guarded by intervening slips of wood and pads. After a few turns of the screw the already weakened tibia yielded; the deformed fibula, however, still prevented the straightening of the limb, but this was easily fractured by a smart blow with the mallet, a small pad intervening to protect the integument from injury. A little manipulation and extension now sufficed to bring the limb into good form, when the wound was "sealed" with Friar's balsam, and the fracture "put up" in one of the ordinary box splints of the hospital. The falling sides of this were provided with a couple of long slots to allow the passage of a broad bandage which, with an intervening soft pad, graduated in thickness to make the requisite degree of pressure, afforded a simple but most effective means to steady the parts and counteract any tendency to displacement of the newly adjusted bones. The progress of the case after the operation was throughout most satisfactory. The patient rested well the night subsequent to it, getting but two doses of five minims each of Battley. A very slightly feverish condition was present for a couple of days, and then entirely subsided. Twelve days after the operation, when the apparatus was opened for the first time, the limb was found to be in excellent position, with only a few drops of pus on the site of the wound. In ten days more good firm union had occurred, with a perfectly straight limb. A couple of days later a light plaster of Paris bandage

^a The instrument was one of the ordinary forms of clamp used by chair makers, &c., to bind together freshly-glued joints. It may be described as consisting of a C-shaped frame of strong iron, one end being flattened, and the other traversed by a strong screw, worked through a handle at its upper free end. The lower end of the screw being furnished with a collar and attached flat surface between which and the flat surface on the lower limb of the C frame, any object can be held grasped with an immense force by working down the screw sufficiently. These affairs, which can be purchased at the artists' warehouses, of various sizes and for a few shillings, answer to the full all the purposes of a very similar apparatus described as a special instrument for re-fracture of bone.

was applied, the girl allowed to get up and move about on crutches. She was discharged in one month and three days after the operation, with her limb of the same length as its fellow and indistinguishable from it in appearance. The result was in every way all that could be desired.^a When subsequently seen the girl knew of no difference between her two legs. It may be mentioned that in effecting the bone section a considerable portion of the edge of the too highly tempered steel chisel broke off deep in the wound. This could not be extracted at the time, and was not discharged afterwards, nor did its presence seem to be productive of any effect whatever.

CASE II.—*Angular Deformity following Fracture of "both Bones" of the Leg; Re-fracture by Screw-clamp; delayed union; ultimate good result.*—T. C. (No. 2,686), seven months old at the time of his admission to hospital; five months previously both bones of the right leg were broken nearly three inches above the ankle, in consequence of a fall. Firm ossific union resulted, but with an amount of deformity which in degree nearly equalled that in the preceding case, though the excessively fat state of the infant somewhat masked the appearance. The limb, from the knee down, proved by measurement to be more than an inch shorter than its fellow. Three days after admission chloroform was given, when the bone proving too firm to be dealt with by the hand, the screw-clamp used in the preceding case was applied, and speedily broke through the tibia. The fibula was then broken by the mallet, with soft pad intervening, in like manner as was done to M. M. A little sustained moderate extension now brought the leg down to a good position. There was no appearance of tension or strain on the muscles on the back of the leg, or on the tendo Achillis, wherefore this structure was not interfered with. The fracture was "put up" in a suitably padded pair of specially made miniature Cline's side-splints; the inner one, having a longer than ordinary foot-supporting portion, was first applied, and secured above and below by broad straps of adhesive plaster. A short hollowed splint was also used posteriorly, as well as a smooth pad of folded lint in front over the region of the fracture, all being secured snug by a roller bandage. In this manner, notwithstanding the infant's tender age, and the necessity of its being constantly borne in the arms, absolute immobility and the necessary rest for the parts was secured. In six days the dressings were soiled, so as to necessitate renewal; the parts were found lying in excellent position. The limb was "put up" precisely as before, and did not require further change for another six days, when also its condition appeared all that could be desired, repair being fairly advanced, having regard to the brief lapse of time since the operation. It would seem, however, as if about this epoch a halt, coincident with a period of markedly depreciated health, came in the reparative powers of the infant,

^a Vide Fig. 2 on lithographic plate.

who, a few days later on, began to lose in appearance, and to become pale and anæmic. Suitable ferruginous tonics were not responded to, and the little patient daily growing worse, was, in view of the benefit likely to attend the change, a month after the operation taken by his mother to their home in the county Kildare, the limb lying in faultless position, but with no appreciable advance towards further consolidation.

Ten days later, when brought back to the hospital, the infant was found to be labouring under well-marked whooping-cough. Whilst at home the dressing had shifted, and a slight degree of angular deformity had been reproduced. Solidification had advanced to some inconsiderable degree, but still allowed of motion; the parts, however, were, with a moderate degree of pressure, easily gotten again into a good position. For a time the tendency to reproduction of deformity at the seat of fracture required constant care and watchfulness to counteract it. Further improvement progressed but slowly and fitfully, and it was not till thirteen weeks after the operation that the fracture was sufficiently united to admit of the patient's departure from the hospital. He then had a straight and shapely limb, though some tendency of the bone to "give" could be elicited on careful manipulation. As a measure of precaution, short light lateral splints were directed to be worn for a time. During the treatment due attention was paid to the administration of tonics, to dietetic and hygienic arrangements. Since his return home the child was reported to have progressed in every way satisfactorily. The splints were discarded in a couple of weeks, the limb then being perfectly solidified, and in good position. Owing to the restlessness of the little creature, no photograph could be obtained representing its condition before the operation.

USES OF BROMIDE OF POTASSIUM EMPLOYED EXTERNALLY.

MM. FERRAND and Gueneau de Mussy employ bromide of potassium, the first in glycerine to combat local spasms, the second in pomade to relieve pruritus. M. Peyraud goes further and finds it for surgical purposes a good derivative, and even a caustic. In 28 days he succeeded, by its daily application as a powder, in destroying an epithelioma growing on the face. He has obtained good results by using it topically in cases of atonic ulcers. He uses it either in powder or in an ointment (1 to 5 parts in 10 of glycerine). In chronic eczema, pityriasis, acne, and other cutaneous affections, as well as in ulcerous stomatitis, he has found it beneficial. As a hæmostatic in epistaxis he has found a solution (1 in 15) serviceable, where perchloride of iron and other styptics had failed.—*L'Union Médicale*. [Cf. "Report in Therapeutics," by editor of "Periscope," *Irish Hosp. Gaz.*, Vol. II., p. 319.]

S. W.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

*Of Eight Large Towns in Ireland, for Four Weeks ending Saturday,
March 23, 1878.*

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	
Dublin, -	314,666	795	749	32	9	11	2	18	31	11	31·0
Belfast, -	182,082	524	422	8	12	5	3	3	24	11	30·3
Cork, -	91,965	212	211	—	11	—	—	4	4	11	29·8
Limerick, -	44,209	79	100	1	—	2	—	—	1	4	29·3
Derry, -	30,884	86	64	—	—	1	1	—	—	2	26·8
Waterford, -	30,626	65	101	—	8	—	—	15	—	—	42·8
Galway, -	19,692	35	35	—	—	—	—	—	2	—	23·3
Sligo, -	17,285	33	31	—	—	—	—	1	2	2	23·3

Remarks.

A general diminution in the death-rate compared with that prevailing in the previous four weeks is noticeable. In Waterford the mortality was once more extremely great. The death-rate was 23·5 per 1,000 of the population annually in London, 25·7 in Glasgow, and 26·3 in Edinburgh. Omitting the deaths of persons admitted into public institutions from localities outside the registration district, the rate in Dublin was 29·7 per 1,000; within the city itself it was 32·3 per 1,000. The deaths from zymotics numbered 142, compared with an average of 131 in the corresponding period of the previous ten years. Small-pox was *more than three times as fatal* as it had been in the preceding four weeks. Measles and whooping-cough decreased. Scarletina and fever increased considerably. Of the 31 deaths ascribed to fever, typhus caused 8, enteric 18, and continued fever of undetermined type 5. The admissions of small-pox patients to Cork-street Hospital were 93, compared with 61 in the previous four weeks. The epidemic rages principally on the south side of the Liffey. An outbreak of small-pox has also occurred in Belfast, causing 8 deaths in the four weeks under discussion. In that

town fever and measles are also still prevalent and fatal. In London small-pox caused 183 and measles 153 deaths, thus showing a slight increase of the former and a decided decrease of the latter disease. The deaths from respiratory affections in Dublin were 179 (average of previous ten years=187·1), including 128 from bronchitis (average=148·1) and 36 from pneumonia (average=21·5).

METEOROLOGY.

*Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W.,
for the Month of March, 1878.*

Mean Height of Barometer,	-	-	-	30·140 inches.
Maximal Height of Barometer (on 16th at 9 p.m.),	-	-	-	30·712 „
Minimal Height of Barometer (on 31st at 9 p.m.),	-	-	-	29·313 „
Mean Dry-bulb Temperature,	-	-	-	43·8°
Mean Wet-bulb Temperature,	-	-	-	41·4°
Mean Dew-point Temperature,	-	-	-	38·5°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·234 inch.
Mean Humidity,	-	-	-	81·0 per cent.
Highest Temperature in Shade (on 1st),	-	-	-	59·6°
Lowest Temperature in Shade (on 24th),	-	-	-	29·7°
Lowest Temperature on Grass (Radiation) (on 30th),	-	-	-	25·2°
Mean Amount of Cloud,	-	-	-	64·6 per cent.
Rainfall (on 17 days),	-	-	-	1·157 inches.
General Direction of Wind,	-	-	-	N.W. and W.

Remarks.

Three distinct types of weather were experienced during this month. Until the 12th the barometer stood high over France and the Bay of Biscay, while a series of atmospherical depressions passed—at first from W. to E., and afterwards from N.W. to S.E.—across Scandinavia and the northern parts of the British Isles. Owing to the great disparity in the height of the barometer in the N. and in the S., very strong westerly to north-westerly gales swept over the N.W. of Europe. Comparatively little rain fell in Ireland, where also the temperature was high for the season. From the 12th to the 21st an anticyclone lay over the United Kingdom, so that the weather became cooler with a very gentle polar air-current. In Dublin, the sky having cleared and the air being calm, some sharp night frosts occurred, followed by smoke fogs in the mornings. The barometer rose to 30·712 inches at 9 p.m. of Saturday, the 16th. The centre of the anticyclone lay to S.W. or S. of Ireland from this day, so that the weather remained quiet and settled, while it became milder. On the afternoon of the 21st, however, a complete change set in, owing to the advance of a deep atmospherical depression across the N.W. of Europe from the Atlantic. It ushered in a period of bitterly cold polar

winds with frost, hail, and snow. From the 23rd the Dublin mountains remained white with snow, and the mean temperature of the week ending Saturday, the 30th, was—even in the city—37·7°. This very wintry weather was caused by the passage across North-Western Europe of several polar cyclonic systems. In Dublin snow or hail fell on the 22nd, 23rd, 24th, 25th, 27th, 28th, 29th, and 30th; while the exposed thermometer on the grass marked several degrees below freezing point on every night from the 22nd. The air was often very dry and parching during this period—thus the relative humidity was so low as 44·5 per cent. at 3 p.m. of the 29th. On the afternoon of Sunday, the 24th, a polar bourrasque travelled down the E. coast of England from N., and caused the foundering of H.M.S. *Eurydice* off the Isle of Wight in a severe snow-squall about 4 p.m.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

PROGRESSIVE PERNICIOUS ANÆMIA.

IN a paper on the above subject, in the 3rd and 4th numbers of the *Nordiskt Mediciniskt Arkiv* for 1877, Dr. S. T. Sörensen reports eleven cases of the disease, which had come under observation in the Kommune Hospital, in Copenhagen, within a period of little more than a year and a half. Of these, seven were men and four women. The disease was fatal in every instance, and *post mortems* were obtained in nine cases. In ten cases out of the eleven, observations on the number of red blood corpuscles were made from time to time by Malassez' method; and these showed a diminution to a number varying from one-quarter to one-twelfth of the normal. Successive examinations showed a continual decrease in number, the last observation prior to death giving from 0·79 to 0·45 millions per cubic mm. The author concludes that 0·5 millions per cubic mm. is the minimum possible for the conservation of life. The individual red corpuscles presented abnormalities in colour, size, and shape; the colour was less intense than in health; some were twice, and others one-half the normal diameter; and in all sizes there were irregularities in form. The reaction of the serum was normal, and when added to healthy blood produced no pernicious effect on the red corpuscles. The prominent symptoms of the disease, according to the author, are—*anæmic* symptoms; gastric disturbance; a pale, yellowish, but not icteric, hue; a considerable degree of fat, notwithstanding the great debility; *bruits de souffle* in the heart and vessels of the neck; retinal hæmorrhages; the occurrence of irregular and unaccountable attacks of pyrexia. The result of the *post mortems* was briefly as followed. The

blood, which was excessively thin, presented the appearance of meat-washings. There was granular and fatty degeneration of the parenchyma of various organs—viz., liver, kidneys, and adrenals. Fatty degeneration of the heart, and atheroma of the aorta. There were likewise, in some cases, numerous capillary hæmorrhages. The marrow of the bones was noticed in four cases only, and in but one of these was a microscopic examination made. In two of those in which it was examined macroscopically only, the marrow of the ribs was of a reddish colour, and of a thinly fluid consistence. In the third case that of the sternum appeared normal. In the fourth case—that examined microscopically—the compact tissue of the ribs was thickened, and the medullary spaces almost empty. The marrow was in no place fatty, while in another it contained some blood corpuscles and a few transitoria cells. Eleven cases occurring in so short a period of time would point to the disease being anything but a rare one. Further, the statistics do not uphold the generally received opinion, that it more frequently attacks females than males. No connexion could be traced in any of the four female cases to pregnancy or child-bed, nor in any of the entire number to unhealthy occupations or privation. One patient, however, attributed her illness to the fatigue of nursing a sick sister, and grief at her death. The author regards the disease as dependent on a mal-production, not a destruction, of the red corpuscles; and he attaches great importance to the numerical examination of them in the diagnosis of the disease. Transfusion of human blood, employed in one case, was not productive of any good.

R. J. H.

INHALATION OF SALICYLIC ACID IN CROUP.

R. OTTO, believing that croup is due to the presence of vegetable parasites, has employed the inhalation of a solution of salicylic acid (1 part in 50) in an epidemic of this disease which prevailed in Lavonia in the autumn of last year. The treatment was adopted in seven cases, being applied as soon as the convulsions appeared. In all the cases the number of the paroxysms very rapidly diminished and excellent recoveries were made. The author does not claim to have found a specific, but he hopes the remedy may be tried on a larger scale.—*St. Petersburg Med. Wochenschrift*.

S. W.

MEANS OF ARRESTING EXCESSIVE VOMITING IN PREGNANCY.

COPEMAN (*Brit. Med. Jour.*, 1875, i. 637) discovered that dilating the neck of the uterus, either with the finger or with a suitable instrument, completely answered this end. An Italian accoucheur, finding his patient's life imperilled by vomiting that resisted all treatment, resolved to cause abortion, and with this view dilated the cervix with his finger. Before proceeding to break the membranes he delayed about half an hour, and

at the end of that time found that the vomiting had entirely ceased, and never re-appeared. The woman was confined at the usual time. In two other cases the expedient was tried, and in both successfully. Another plan is to administer iodine internally—half a drachm of the tincture is mixed in eight ounces of water, and a tablespoonful taken every hour.—*Gazetta Medica di Roma*, and *Revue Médicale*. S. W.

NEW PREPARATIONS AND SCIENTIFIC INVENTIONS.

THROUGH the courtesy of Messrs. Seabury and Johnson, of 28, Red Lion Square, Holborn, London, W.C., and New York, we have been favoured with samples of the India-rubber,*pharmacopœial, medicinal, and mechanical plasters manufactured by that eminent firm.

The mustard, capsicum, belladonna, adhesive, and “blister” plasters are both efficacious and cleanly; and the same remark applies to the porous plasters of opium, belladonna, capsicum, pitch, and cantharides (“warm plaster”).

Of the whole series, that with which we are most pleased is the “Salicylated Isinglass Plaster.” Messrs. Seabury and Johnson point out that fish isinglass, when perfectly pure, is undoubtedly the best and most innocent vehicle for surgical plasters in order to secure adhesion. The mode of manufacture of their adhesive plaster is simple, and will itself furnish the best explanation of its qualities and value. The meshes of the strong cotton cloth, which forms its basis, are impregnated with India-rubber; this adds new strength to the already strong material, in addition to rendering it perfectly waterproof. A solution of chemically pure gelatine is then charged with a sufficient quantity of salicylic acid, and spread on the waterproofed cloth, thereby rendering it non-irritating, antiseptic, very susceptible to the slightest moisture, and very adhesive.

For the varied operations of the surgeon in private or hospital practice, this plaster deserves special recognition. It can be applied almost instantly, without heat. The application of moisture conveyed in any form—the most convenient medium being that of a wetted sponge—causes it to adhere quickly without any danger of displacement. If the surgeon carefully wets the surface of the plaster, or draws it through warm or cold water, it will adhere very firmly and closely. Sometimes, indeed, the plaster tends to become hard, and therefore uncomfortable; but this objection applies more or less to all adhesive plasters. Being waterproof, wounds can be cleansed without re-dressing, whilst the mild antiseptic action of the incorporated salicylic acid tends entirely to prevent putrefaction. It is said to remain serviceable in any climate for years.

Finally, its moderate price is not the least important point in its favour. It can be had in 5-yard rolls, $11\frac{1}{2}$ inches wide, for six shillings; or in yard rolls of the same width for eighteen pence.

THE DUBLIN JOURNAL

OF

MEDICAL SCIENCE.

JUNE 1, 1878.

PART I.

ORIGINAL COMMUNICATIONS.

ART. XV.—*On the Anatomy and Physiology of the White Tracts of the Spinal Cord.*^a By J. M. PURSER, M.D., F.K.Q.C.P.

II.—PHYSIOLOGY.

OUR knowledge of the functions of the fibres in different tracts of the spinal cord is derived partly from physiological experiments performed on animals, partly from clinical and pathological observations made on man.

Notwithstanding the zeal and ability with which each of these methods of research has been prosecuted, there is still a sad, although continually decreasing, difference of opinion among physiologists as to the parts which should be assigned to the various white tracts and portions of grey matter in the transmission of centripetal and centrifugal impressions.

The reasons for the want of unanimity are not difficult to find.

When we consider the inaccessible position of the cord and its small size in those animals which are most used for experimental purposes, we can readily understand the difficulty which is experienced in irritating or dividing isolated tracts of fibres or portions of grey matter. But this difficulty is small in proportion to that of interpreting the phenomena exhibited by the animal during and

^a A Discourse delivered before the King and Queen's College of Physicians, February 25, 1878.

after the experiment, and of separating those which are the mere result of shock, hæmorrhage, pain, &c., from those which are due to the section or irritation of the cord.

In diseases of the cord in man, when the symptoms, particularly those referable to sensation, can be so much better studied than in the case of animals, the morbid changes are generally so badly localised that it is not easy to say which of the symptoms are to be attributed to alteration of one part and which to another. Besides, it is often a matter of the greatest difficulty to distinguish between the effects of irritation and those of destruction of the nervous centres. This was never so much the case as now when we know the large part played in nervous processes by inhibitory apparatus. We shall see that such symptoms as hyperæsthesia, spasm, &c., which were formerly always attributed to irritation, may be often due to paralysis or destruction of inhibitory fibres, and that anæsthesia or paralysis may in like manner be caused as well by irritation of one part as by destruction of another.

Still, notwithstanding all these difficulties, a steady advance in our knowledge of the functions of the nervous centres is taking place, and coincidently with this, partly as cause, partly as consequence, a great increase in our power of understanding their diseases. In no other instance has medicine benefited more by the labours of the physiologist, and the physician has amply repaid what he received from science by furnishing to the physiologist facts drawn from observations on man, and which could never have been discovered by experiments on lower animals.

The relative share which is taken by the white and grey matter of the cord in the transmission of centripetal and centrifugal impressions has been from the first one of the most hotly disputed points in physiology. While Longet and Van Deen (in his earlier works) attributed the function of conduction exclusively to the white columns, Stilling took an almost equally extreme view in the contrary direction, and made the passage both of sensitive and motor impulses to occur mainly in the grey matter. Recent observers have adopted a somewhat middle course, and admit that both parts take an important share in the function of conduction.

The experiments of Brown-Séquard and of Schiff are those which have most profoundly influenced the question of conduction through the spinal cord. As is known, Brown-Séquard, while admitting that the grey matter has some share in conveying motor impulses, since its partial division causes feebleness posterior to the section,

attributes to the antero-lateral white columns the principal part in the conveyance to the muscles of the orders of the will. Passing through the anterior pyramids of the medulla oblongata, the motor conductors decussate at the decussation of the pyramids, and run in the cervical region, mainly in the lateral tracts, while in the lower parts of the cord they are contained, for the most part, in the anterior columns. Motor conduction thus occurring in the white matter must necessarily follow fixed paths, for we have every reason to believe that an impression never passes laterally from one nerve-fibre to another. Whether the conductors of motor impulses decussate completely in the medulla oblongata, or whether a crossing of fibres occurs all along the cord, is a question that Brown-Séquard thinks must be answered differently for men and for lower animals. It is his opinion that pathological cases prove positively that in man the decussation at the pyramids is complete, while in animals he finds, after a hemisection of the cord, a certain power of movement in the posterior limb on the side of section. In this case, therefore, it would appear that the decussation in the pyramids is not complete, but that a crossing of conductors takes place in the cord also. According to Van Kempen, who was one of the first to notice the crossing of motor conductors in the cord, this decussation is best marked in, if not altogether confined to, the cervical region. Vulpian, however, finds that irritation of the anterior column on one side in the dorsal region causes contraction in the posterior limbs on both sides, and this when every care is taken to prevent the extension of the irritation to other regions than that to which it is immediately applied.

Schiff, while agreeing in the main with Brown-Séquard as far as concerns *voluntary* movement, holds that motor conduction of an involuntary and reflex character can be effected across a point of the cord at which all the white fibres have been divided, and only grey matter remains, and that it is of no consequence what part of the grey matter is left, as in it the impressions spread out in all directions, and do not follow fixed paths as they do in the white columns.

As regards sensitive impressions, a share in their conduction has been given even by very early experimenters to the grey matter. But Longet deprived it of this function, which he attributed altogether to the posterior columns. Bellingeri, Fodéra, Stilling, and others had, however, found that not only did sensation persist after division of the posterior columns, but that it was increased in the

parts behind the section—that the operation, in fact, produced not anæsthesia, but hyperæsthesia. Brown-Séquard confirmed this, and still further disproved the exclusive conduction through the posterior columns, by dividing all the parts of the cord with the exception of these, when the loss of sensation in the parts behind the section was as complete as if the entire medulla had been severed. Brown-Séquard and his followers found, furthermore, that not only the posterior, but also the other white columns must be excluded from the function of centripetal conduction, for if all the white matter of the cord is divided, leaving a bridge of grey substance, sensation still persists, while section of the grey substance, leaving the white intact, causes complete anæsthesia. So far as the mere fact that sensitive conduction takes place through the grey matter, Brown-Séquard and his most illustrious disciple, Vulpian, are at one, but beyond this a serious difference occurs. While Brown-Séquard makes sensation follow fixed paths, which all decussate in the cord at a short distance from the point at which the posterior roots enter, Vulpian^a affirms that conduction in the grey matter takes place in all directions, and that if a lateral section of the cord be made on one side in the dorsal region, and another similar section on the other side at some distance higher up, there is no anæsthesia of any part, although in all the posterior parts of the body sensation may be enfeebled. Brown-Séquard^b has, after the same experiment, seen complete anæsthesia on both sides. The directly contrary results obtained in such an apparently simple observation by two such masters of experimental art, show in the strongest light the difficulties which beset on every side the physiological investigator.

Schiff's views on sensation are in many respects widely different from those of Brown-Séquard. He finds that the conduction for tactile impressions takes place exclusively in the posterior columns, follows fixed paths, and does not undergo any decussation in the cord. Painful impressions, on the other hand, travel through the grey matter, and not in fixed paths, although in his later works Schiff admits that in man conduction is crossed.^c He finds that if the posterior columns be preserved, while the grey matter is destroyed, the animal manifests distinct signs of feeling every time

^a *Système Nerveux*. P. 390.

^b *Lectures on the Physiology and Pathology of the Central Nervous System*. P. 32.

^c Conduction of painful impressions is crossed in men, rabbits, and dogs; uncrossed in cats.

the posterior parts of the body are touched, however lightly, while the nerve-trunks may be exposed and crushed, or powerfully galvanised, without causing any evidence of pain, showing that a condition exists not of anæsthesia, but of analgesia. If, on the other hand, the posterior columns be divided, while the grey matter remains sound, every impression produces signs of pain. It would appear that the tactile conductors enter the posterior column only at some considerable distance above the point at which they pass into the cord. Thus in the lumbar region the conductors of touch from the lower limbs are found in the lateral, not in the posterior columns, which in this part of the cord contain the tactile conductors only from the anus, sexual organs, pelvis, and tail. These views of Schiff, although, as we shall see, they receive some support from the facts of pathology, have met with much opposition from physiologists. Neither Brown-Séquard nor Vulpian has found tactile sensibility to persist after division of the entire cord, with the exception of the posterior columns; and Vulpian thinks that the condition in which every impression is felt as pain is best explained by some modification of ordinary conductors, as he has himself noticed in experiments in which the peripheral nerves were compressed. Brown-Séquard admits separate conductors for each species of sensation, but makes them all (except those for the muscular sense, which do not decussate) run in the grey matter of the opposite side. But Vulpian^a refuses to recognise separate paths for the conductors of touch and those of pain, and inclines to accept Dr. R. M'Donnell's^b beautiful and ingenious hypothesis that the various kinds of sensation are due to different modifications in the same conducting fibres, analogous to the waves of different length which all, affecting the same ether, produce in us the sensations of heat and colour.

As to the manner in which conduction through the grey matter is effected, there seems to be a difference of opinion. Brown-Séquard and those who suppose that it follows fixed paths would make impressions travel along the *fibres* which, as are known, occur abundantly in this part of the cord, while those writers who admit that an excitation once conveyed to the grey substance spreads out in all directions, would throw the chief part of the conduction on the nerve-cells, which, anastomosing by their processes, are

^a Dict. Encycl. des Sciences Médicales. 2nd Series. T. VIII. Art., Moelle Epinière. Physiologie.

^b Proc. Royal Irish Academy. 1870.

supposed to form a continuous network or spongy tissue throughout the grey matter. The anastomosis of the processes of the cells, which was at first assumed on very insufficient evidence, and then considered to be extremely doubtful,^a is now fully established, and shown to be effected not only by the very fine terminations of the protoplasm processes, as described by Gerlach, but also by thick connexions which can be followed without uncertainty.^b

It will be seen from the experiments just described that the anterior columns of the cord have shown themselves in Vulpian's hands not only capable of conducting, but also excitable, like peripheral nerves. He has found the other white columns also excitable, as have the majority of modern investigators, as Fick, Engelken, Dittmar, Gianuzzi, &c. Vulpian, however, denies the excitability of the grey matter, which he still looks on as *æsthesodic* substance in the sense of Schiff.

In later years a series of researches have been carried on under Ludwig's direction at Leipzig, which give results pretty concordant among themselves, but which differ a good deal from those of previous observers. In these experiments a greatly improved method of making the sections of the cord has been adopted, and it may safely be said that never before were sections made with such accuracy and such avoidance of injury to neighbouring parts.

In these observations the question of conscious sensation was left out of consideration, and it was sought to determine whether an impression made on one side of the section could produce an effect at the other. If this was the case, it is clear that fibres (or other parts) capable of conducting across the injured point must remain undivided.

It is well known that when a sensitive nerve is irritated, an increase in the general blood pressure takes place, due to a contraction of the smaller vessels produced by reflex action through the vaso-motor centre in the medulla oblongata. The investigation of the paths in the spinal cord, along which the centripetal impressions are conveyed to this centre, was undertaken by Miescher.^c His experiments were made on the lumbar portion of the spinal cord of rabbits, between the last dorsal and third lumbar vertebræ. He found that in this part of the cord the so-called *pressor* fibres

^a Deiters. *Gehirn und Rückenmark*, 67.

^b Willigk. *Virchow's Archiv*. LXIV., 163. Carrière. *Archiv. f. Mik. Anat.* XIV., 125.

^c Ludwig's *Arbeiten*, 1870.

from the sciatic nerves ran for the most part in the lateral columns, and that both sciatic nerves were represented in each side of the cord, but that the majority of the fibres lay on the opposite side—or, in other words, that the fibres in question at the point examined had to a large extent, but not completely, undergone decussation.

In these experiments it was left to some extent undecided whether some of the centripetal fibres from the sciatic did not run in the grey matter. Nawrocki,^a taking up the investigation, showed that this was not the case, but that in the upper lumbar portion of the cord the pressor fibres were all contained in the lateral white columns.

Dittmar,^b continuing the inquiry, found that not only did the centripetal fibres going to the vaso-motor centre run in the lateral columns, as proved by Miescher and Nawrocki, but that the centrifugal fibres conveying the impressions from this centre to the vessels ran also in the same tracts.

Worschiloff,^c leaving the vascular nerves, sought to determine the tracts of the cord through which passed in the lumbar region the motor impulses for the muscles of the lower limbs, and the centripetal impressions from the posterior part of the body, which call forth movement in the upper limbs. The methods employed were these:—After the section of the cord the animal was examined in three ways—1. It was seen whether pinching or other stimulus applied to the posterior limbs caused movement in the upper parts of the body. 2. Whether in sitting or running the animal had the power of moving normally or otherwise the hind limbs. 3. Whether on galvanising the cervical cord below the medulla oblongata, the normal co-ordinated movements in the posterior limbs were obtained. (These consist not in tetanic movements, but in a series of flexions and extensions, perfectly well co-ordinated, and similar to those made by the animal in ordinary progression.)

The experiments were always made in pairs—1. A certain tract of the cord was divided and the effect noted. 2. In another animal the part divided in the first experiment was untouched, all the remainder of the cord being severed. All experiments were made on rabbits at the level of the last dorsal vertebra. It was found that section of the anterior and posterior columns, together with the grey matter, was without effect. Irritation of the posterior limbs continued to call forth reflex movements in the anterior, and

^a Ludwig's Arbeiten, 1871.

^b Ibid., 1873.

^c Ibid., 1875.

vice versâ; the animal sat and moved normally, and co-ordinated movements occurred when the cervical medulla was stimulated. Section of the two lateral columns, on the other hand, leaving intact the parts previously divided, prevented the propagation of impulses across the seat of injury. Section of one lateral column, with or without that of the central columns and grey matter, left both legs sensitive and capable of motion; but irritation of the foot on the side of section produced strong movements in the upper limbs (hyperæsthesia), while only feeble contractions in these were called out by irritation of the hind limb on the sound side (anæsthesia). Hence each lateral column contains centripetal (sensitive) fibres from both limbs, but more from that of the opposite side than from that of its own (decussation of sensitive conductors). Both hind limbs possessed the power of movement, but these were (whether spontaneous or provoked by irritation of the anterior part of the body) stronger on the sound side than on that of section. Hence each lateral column contains motor fibres for both limbs, but most for that of its own side—*i.e.*, motor conductors decussate only to a slight degree in the cord.

It will be seen that these works from the Leipzig Institute differ from those previously published, chiefly in showing that at least so far as the lumbar region of the cord in rabbits is concerned the lateral tracts, which have hitherto received far less attention than the anterior and posterior columns, are nevertheless the only parts in which fibres lie which connect with one another far distant parts of the nerve centres.

Before we pass to the facts of human pathology we may say a few words about the hyperæsthesia which follows certain injuries to the brain or cord. This occurs after hemisection of the cord or medulla oblongata, and affects the body on the side of section. It also follows more limited injury, but on this head there is much discrepancy. According to Brown-Séquard, Vulpian, and many other writers, it is very marked after section of the posterior columns, with or without section of the posterior grey matter.^a Worschiloff, however, found after this operation no signs of hyperæsthesia. Miescher, too, failed in any of his experiments to observe increase of sensibility. Vulpian and Brown-Séquard state that section even is not necessary, but that a mere puncture of the posterior columns with a needle is sufficient to cause hyperæsthesia on the same side and anæsthesia on the opposite, these two conditions almost always

^a Loc. cit. Explanation of Plate III.

going together; but Vulpian^a himself, in an experiment made for another purpose, punctured the posterior column in a guinea-pig with a needle charged with glacial acetic acid. The animal manifested scarcely any pain, and neither at the time nor subsequently was there paralysis, anæsthesia, or hyperæsthesia in the posterior limbs. Worschiloff has shown that in order that the phenomena of hyperæsthesia should manifest themselves, the inner part of the middle third of the lateral column (from before backwards) must be divided. Wilhelm Koch^b has more recently studied for each region of the cord of rabbits the parts that it is necessary to divide in order to cause hyperæsthesia. He finds that these lie in the lateral column, and that section of the grey matter is ineffective. This observer also finds the remarkable fact that an animal whose cord has been partially divided continues, even after removal of its cerebrum, to manifest the signs of hyperæsthesia, not only by movements, but even by screaming, as if in severe pain. He therefore places the entire phenomenon among those of reflex character. That, however, an increase of conscious sensation occurs in these cases is certain, for it has frequently been noticed in man after injury or disease of the cord. The explanation is, however, extremely difficult. The theory which seems best to meet the requirements of the case is that from each part two sets of centripetal fibres ascend in the cord—one, which, for the most part, decussates, conveys to the brain the sensitive impressions and to the upper part of the cord, &c., those which are here reflected on motor nerves; the other, which, for the most part, does not decussate, has a moderating or inhibitory influence on the centres called into activity by the fibres of the first set. If, then, we suppose the right half of the cord divided, the right foot will have lost most of its inhibitory, but retained most of its sensitive fibres, consequently it will be hyperæsthetic; the left foot, on the contrary, will have retained most of its inhibitory, but lost most of its sensitive fibres, and will be therefore anæsthetic. If, however, it be true that mere puncture will cause hyperæsthesia, this explanation will not suffice. We must then suppose some peculiar condition of increased irritability produced by the puncture, but why this should be associated with diminished irritability on the opposite side is not very clear. The occurrence of the condition after puncture seems to me, however, to need confirmation.

^a Archives de Physiologie. 1869. 234.

^b Archiv. f. Anat. und Physiologie. 1877. Physiologische Abtheilung. 475.

There is great difficulty in making use of the data derived from human pathology for the elucidation of the paths of the conductors in the spinal cord. In acute cases of spinal disease the lesions are seldom limited to one tract or region of the cord, and it is frequently impossible to explain the symptoms separately, and to say which is caused by irritation or destruction of one set of fibres or cells, and which by lesion of another. In cases of chronic disease the course of the affection is frequently so lengthened that the patient passes through the hands of many physicians before his death, and the history in all its stages is seldom accurately known. Besides, in these cases, too, the morbid changes, although at first they may have been limited to one part of the cord, are generally more or less diffused before the examination can be made. It seems to me that an unnecessary complication has been introduced into the question by the indiscriminate quoting of published cases, and by the attempt to draw conclusions from observations made long ago when the methods not only of pathological research but of clinical examination were so imperfect that the facts of diseases of the nervous system recorded then are quite useless to us with our present knowledge. It is not that I think the old physicians were worse or more careless observers than those of the present day—far from it. But I hold that it would be as impossible for them to have defined and localised the structural changes in the cord without the use of the microscope, or to have described the alterations in functions (such as those of the vaso-motor nerves) of whose very existence they had no idea, as it would have been for them to have analysed the photosphere of the sun without the spectroscope, or to have foretold the occurrence of a storm a week before its advent without the telegraph. But it is quite certain that many of the discrepancies are not due to imperfect observation, and the facts which I set forward, when speaking of the development of the cord, show that it is highly probable that the same lesion in different subjects may cause widely different symptoms. We have seen that the course taken by the pyramidal fibres is subject to great variety; that they sometimes completely decussate in the medulla oblongata, and run altogether in the lateral tract of the opposite side, while sometimes they pass for the most part in the anterior column of the same side. Whether these latter fibres decussate in the cord before entering into relation with the anterior roots is unknown, but it is quite possible that they do not, and that many cases of hemiplegia occurring on the side on which the

brain lesion existed may be accounted for by a want of decussation of the pyramids. But whether such spinal decussation occurs or not, it is evident that the destruction of an anterior column, in which the bulk of the pyramidal fibres are contained, must have a very different effect to lesion of one in which, as is usually the case, only a small number are found. Similar considerations will account for contradictions in observations of disease in other parts of the nervous system where crossing of fibres is met with, notably in the case of the optic chiasma, which has been, and is still, a favourite battle-field for the different schools of pathologists.

With regard to the value for human pathology of the facts derived from vivisection, great difference of opinion exists. It is not yet known whether the same individual variety in the course of the fibres as has been found in man exists in the lower animals; but although the decussation of the pyramids is not so complete in them as in the human medulla, yet the close agreement between the secondary degenerations in man and those found by Schieffer-decker in the dog, show that, at all events for this animal, the general arrangement of the white tracts is the same as in us. As it is, there is, on the whole, a very remarkably close agreement between the results got by experiment and those obtained by clinical and pathological observation in men. Those who are sceptical as to the results of experimental physiology I may remind of a remarkable prognostication made by Schiff. This physiologist, from what he had observed in animals, foretold that cases of spinal disease would be met with in men in which every impression made on the lower part of the body would be felt twice—first as a sensation of touch, and subsequently after a greater or less interval, as one of pain—and that in these cases the grey matter would be the seat of disease. Several such cases have been since recorded by E. Remak, Naunyn, Vulpian, and others.

I shall now, finally, notice with all brevity a few of the best characterised diseases of the spinal cord, which are due to alteration in its conducting apparatus.

Except complete division of the spinal cord causing paraplegia and entire loss of sensation, there is no lesion of this organ whose symptoms are so definite, and seem to admit of such satisfactory explanation, as unilateral injury—destruction of one lateral half of the cord to a limited extent. The main features of this condition are now well known, and consist in paralysis of motion, with more or less persistent hyperæsthesia, loss of muscular sense, and, at

first, vascular dilatation on the side of injury—conservation of motion, but anæsthesia for temperature, pain, touch, and tickling on the opposite side. This group of symptoms shows that in man there is decussation of sensitive conductors in the spinal cord, but does not show in what tract they pass to the brain. It shows further that the motor conductors run in the cord without decussating. The great physiologist^a who has established and explained this clinical condition holds that in man the decussation of the sensitive conductors in the cord is complete, but incomplete in animals; also, that in man no decussation of voluntary motor conductors occurs in the cord, while in animals a partial crossing exists in this region. How far further observation will prove this, as regards sensation, it is hard to say. The conduction of sensation is past all comprehension to my mind, but as regards motion we must bear in mind the now well-established anatomical fact that in most cases some, and in many a very considerable number of pyramidal fibres, which are, beyond doubt, the motor conductors, run in the cord without having undergone decussation in the medulla oblongata. These fibres must either decussate in the cord, or put parts of the body in motor connexion with the brain of the same side, unless we suppose them to decussate higher up than the decussation of the lateral pyramidal tracts. In future a careful examination of the arrangement of parts at the decussation of the pyramids will be an essential matter in all autopsies.

The peculiar condition, probably a chronic inflammatory process, known as sclerosis, has a tendency to affect localised portions of the cord, sometimes as spots scattered through various parts, sometimes confined to certain tracts or columns. From the first, or disseminated sclerosis, we can gain no information as to the course of the conductors; the lesions are, as a rule, too widely scattered over different parts of the nerve centres, and the symptoms consequently too variable to enable us to refer each of the latter to its anatomical cause. We know nothing of the symptoms of sclerosis of the anterior columns, but sclerosis of the posterior and lateral columns respectively give rise to symptoms which are highly characteristic. Sclerosis of the posterior columns constitutes the anatomical basis of the disease known as *tabes dorsalis*, or locomotor ataxy. Of the symptoms of this affection I need say nothing, as they are familiar to all my hearers, but I may notice briefly the pathological anatomy, as it is essential to know that, in order to see how very much less

^a Brown-Séquard. *Lancet*, 1869. Vol. I., page 1.

light than is generally supposed is thrown by this disease on the physiology of the cord.

In an ordinarily developed case we find after death sclerosis^a in the lower lumbar portion of the cord affecting the outer portion of the posterior columns. In the upper lumbar and lower dorsal regions the disease is at its maximum; the entire posterior columns are involved, also almost invariably in greater or less degree the posterior horns of grey matter, Clarke's posterior vesicular columns, and very frequently the inner parts of the lateral white columns. Higher up Goll's columns alone are affected. In the lower part of the cord the posterior nerve roots are atrophic, but only as far as the ganglia, which are sound. Recent researches have shown that in the earliest stage of *tabes dorsalis* the outer parts of the posterior columns (that which we have described as the fundamental tracts) only are affected. The disease extends not only upwards and downwards along these, but transversely so as to involve the grey horns, and Goll's column, which, when affected at one point, then undergoes secondary ascending degeneration. The pain, anæsthesia, and otherwise perverted sensibility are readily explained by the affection of the outer portions of the posterior columns, which are to a very large extent made up of posterior nerve-roots; but to what is the essential symptom of *tabes*, the ataxy, due? That this is not caused simply by the loss of sensation is abundantly proved by the facts (1) that ataxy is often well marked when there is little or no loss of sensation; and (2) that there may be loss of sensibility of every kind, even up to complete anæsthesia and loss of muscular sense, without ataxy, as is proved by the classical case recorded by Späth and Schüppel. The loss of co-ordination must be then a motor phenomenon. Now, we have the strongest reasons for believing that in the higher animals the co-ordination of voluntary movements is effected, not in the spinal cord, but in the higher parts of the nerve centres; hence we must have in *tabes* an interruption of the conductors descending from the co-ordinating centre in the brain to the limbs. What is the path of these conductors? Goll's column, consisting, as we have seen, of long fibres connecting each part of the cord with the higher parts of the nervous centres, would serve anatomically for the passage of such conductors, but nevertheless

^a Without entering into the pathology of sclerosis, we may say it is a condition in which the nerve-fibres waste and disappear, the connective tissue is increased, and the parts affected are diminished in size.

they appear not to have this function, because (1) in secondary degeneration of Goll's column ataxy is not a symptom, and in the few cases recorded of primary sclerosis of these columns ataxy was absent; and (2) cases of well-marked ataxy have been met with where the morbid changes were confined to the outer tracts of the posterior columns. Charcot makes the co-ordinating fibres run in the outer part of the posterior column among the inner bundles of the posterior roots; but anatomically this part of the posterior columns would appear to contain only fibres running a short course, and connecting one part of the grey matter of the cord with another. In fact, as the matter stands, neither can physiology explain the phenomena of locomotor ataxy, nor can it derive from this disease much that is of use in determining the course or functions of the tracts in the cord.

A remarkable group of symptoms has been described recently by Erb and Charcot under the names of spastic spinal paralysis, or spasmodic tabes dorsalis. These symptoms consist in gradually increasing weakness, combined with stiffness of the muscles, great tendency to spasmodic contractions, with remarkable increase of the reflex movements produced by striking or stretching the tendons. There is no pain, no anæsthesia, and the reflex movements called forth by irritation of the skin are scarcely if at all increased. There is no impairment of the functions of the bladder, rectum, or sexual organs, and no wasting of the muscles or other trophic disturbances. The disease usually begins in the lower extremities, and gradually extends to the muscles of the body and upper limbs, but not unfrequently takes the opposite direction, or may even assume a hemiplegic form. It runs an exceedingly chronic course. It has not yet been established by *post mortem* examination that this clinical condition depends on primary sclerosis of the lateral columns, but the evidence for such dependence is overwhelmingly strong—so much so that until the contrary is proved we may assume it to be actually the case. All we learn from this, however, as to the course of the conductors is that the motor fibres run in the lateral column, and that their gradual destruction by an irritative process will cause gradually increasing paralysis with spasmodic symptoms. We do not as yet know in what part of the cord or by what mechanism the tendon reflexes are produced. Their increase in lateral sclerosis is scarcely more remarkable than their absence in ordinary locomotor ataxy.

If from the data derived from the various methods of examina-

tion considered, we attempt now to trace the afferent and efferent nerve paths from the brain, or even from the medulla oblongata, we find that while along part of the tract the way is straight and plain, in others we lose the clue and get into a maze from which we can extricate ourselves only by hypothesis. As my design in these lectures was to set forth what is known, and not to expound the various theories which have been put forward with more or less reason to explain what is unknown, I shall merely indicate the points at which our knowledge ceases.

With regard to the motor conductors, we are in a far better state than as regards those of sensation. We may say with certainty that fibres run uninterruptedly from the grey cortex of the central convolutions of the brain through the lower segment of the crus cerebri, anterior pyramid, posterior part of lateral column of opposite side, and anterior column of same side of cord. In this course these fibres are probably reinforced by others arising from the corpora striata and lenticular nuclei; but, from the facts of secondary degeneration we have every reason to believe that each fibre, from its origin in the brain up to the point where it passes into the grey matter of the spinal cord, runs without interruption by a ganglionic cell, for the interposition of a ganglionic cell usually stops the extension of degeneration along a fibre. The fibres of the pyramidal tract become gradually less and less numerous as we descend the cord; this diminution is caused by successive fibres leaving the white columns and bending into the grey matter. Here we lose all certainty as to their course. We know that they must enter into continuity with the large nerve-cell of the anterior horn, for we have the strongest reason for believing that nervous impulses travel only along structurally continuous paths, and we know that the anterior nerve-roots are continuous with the cells of the anterior grey matter, but more than this we do not know. The integrity of the anterior root fibres, even in the most extreme cases of secondary degeneration of the pyramidal tracts, makes it almost certain that no fibre of the latter leaves the cord without having undergone interruption by a nerve-cell.

The further course of the posterior roots after their entrance into the spinal cord is the subject of much doubt. That they all pass into the grey substance is not at all certain, although highly probable. Schiefferdecker assumes that a considerable part of the fibres pass into Goll's columns, and ascend to the brain without having undergone interruption by ganglionic cells, while those

fibres of the posterior roots which lie in the outer parts of the posterior columns pass more immediately into the grey substance. This view, which is founded chiefly on the appearances met with in Lange's case of compression of the cauda equina, already alluded to, will not, I think, bear examination. The facts of development and of disease show conclusively that the two portions of the posterior columns are altogether distinct, one from the other, and the degeneration of Goll's column found in Lange's case may fairly be looked on as secondary to its destruction in the lumbar region by extension transversely of the irritative degeneration of the lower nerve roots and outer parts of the posterior columns.

Pierret^a fully recognises the two divisions of the posterior columns. He derives the outer portion from the posterior nerve-roots, which all pass sooner or later into the grey substance, while Goll's column is, according to him, commissural between the grey matter of different regions of the cord as high as the medulla oblongata. According to Pierret, Goll's column attains its greatest development in the upper dorsal region. This is, however, in opposition to the measurements of Flechsig, who finds that it increases steadily in size from below upwards to the medulla oblongata. We learn nothing of the functions of this tract from its diseases. Its secondary degeneration gives rise to no symptoms, and in the few cases in which it alone has been found altered the symptoms have not been sufficiently characteristic to give foundation even to a reasonable hypothesis as to its uses. The same must be said of the direct cerebellar tract. There is no serious evidence in support of Schiefferdecker's view that it is the continuation to the brain of fibres from the posterior roots of the opposite side which have passed through the grey matter, undergone interruption by ganglionic cells, and decussated in the posterior commissure. All we can say about these two tracts is that they constitute each a definite system of fibres, which, originating at various heights in the cord, run without interruption to the brain, and that their function is most probably to conduct from below upwards. Of the further fate of Goll's columns beyond the post-pyramidal nucleus, we have only conjectures to offer; and we are equally in the dark as to the true termination of the direct cerebellar tract in the cerebellum.

The outer portions of the posterior columns are composed mainly of nerve-roots on their way upwards and downwards to enter the

^a Archives de Physiologie. 1873. 534.

grey matter; whether any of these fibres pass direct to the brain, or whether, as is much more probable, they all enter into connexion with the cells of the grey matter of the cord, is not yet certainly known.

The remaining tracts of the spinal cord serve, we may say, certainly to connect its different segments with one another, and to unite its upper parts with corresponding regions of the medulla oblongata. These commissures will naturally have to conduct in both directions, and consequently when cut they do not degenerate; for it would seem that although probably every fibre in the cord is in connexion at both ends with ganglionic cells, yet it is only that cell from which it conducts that governs its nutrition, and whose connexion with it preserves it from destruction. It is not impossible that the commissural tracts, besides performing their ligating functions, may serve to propagate sensitive and motor impulses to and from the brain by a series of stages, as a number of men at different heights in a ladder might pass a load from one to another.

It would appear almost certain that, under normal circumstances, the conductors for sensation cross in the cord; that they run in the posterior and lateral columns, and that if these be interrupted, the grey matter is capable, to a certain extent, of conveying sensitive impressions to the brain.

Schiff's theory of double conduction by the posterior columns and by the grey matter receives some support from the cases in which the passage through the latter path is delayed, and in which each impression made on the surface is felt twice—first as a sensation of touch, and later as a painful sensation.

But it is not only in the nervous centres that it is doubtful whether sensation always follows fixed paths. The same doubt may be felt as to the peripheral nerves. Vulpian^a calls particular attention to the fact that when, in tabes, the posterior nerve-roots are much wasted, we do not find, as we should expect, certain parts of the surface corresponding to the atrophied fibres altogether anæsthetic, while other parts preserve their normal sensibility, but that a condition of general impairment of sensation exists everywhere. He supposed that in the spinal ganglia a transference of impressions from one fibre to another might take place, so that a few sound fibres in the posterior roots might be able to convey to the cord the impressions coming to the ganglia from many different parts of the periphery. In accordance with this theory he found,

^a Archives de Physiologie. 1868. 150.

besides considerable delay in the rapidity of conduction, a loss of the power of accurately localising painful impressions. But that this theory will not meet all the requirements of science, is shown by Vulpian himself; for he found that in frogs not only did partial section of the posterior nerve-roots between the cord and ganglia produce such a condition of general enfeeblement of sensation with delay of conduction, but without spots of complete anæsthesia, as he had observed in tabes, but that a precisely similar condition was produced in the lower limbs when their nerve-trunks were partially divided in the abdomen, a long distance at the peripheral side of the ganglia. It is evident from this that the transference of impressions from one fibre to another must be capable of being effected either in the peripheral plexuses or in the nerve-trunks themselves. But, in either case, how is the localisation of sensation to be accounted for? For this and other reasons, I think I did not overstep the truth when I said that, as our knowledge stands at present, the conduction of sensation is past all comprehension.

ART. XVI.—*On the Prevalence of Small-pox in Dublin.*^a By THOMAS W. GRIMSHAW, M.A., M.D.; Fellow and Censor of the King and Queen's College of Physicians; Physician to Steevens' Hospital; Consulting Physician to Cork-street Fever Hospital; Examiner in Medicine to the Queen's University in Ireland.

IN bringing a paper on the present small-pox epidemic under the notice of the Medical Society of the College of Physicians, I am aware that some consider I am premature in discussing the question of the epidemic at so early a date. I consider, however, that the great importance of the questions—how the epidemic arose; how it has been promoted; how it may be ameliorated; and how future epidemics of the same sort may be prevented, warrant the remarks I am about to make.

Previous to the autumn of 1876, Dublin was almost free from small-pox, since the close of the epidemic of 1871–3. In 1876 the disease made its appearance in the towns of the north of England

^a Read at a Meeting of the Medical Society of the King and Queen's College of Physicians, on May 1st, 1878, as the opening of a discussion on the present small-pox epidemic in Dublin. [For the discussion on this paper, see p. 531.]

which are in daily communication with Dublin, and thence, as I shall show presently, it was introduced into Dublin, where it found a ready soil for its propagation in consequence of the unsatisfactory state of vaccination.

Before I proceed further, I wish to refer to the relations between the prevalence of small-pox and the progress of the practice of vaccination in Ireland and in Dublin. Statistics compiled from the reports of the Census Commissioners (see Table I.) are very remarkable, and show in a most conclusive manner the great benefits which vaccination has conferred on the community.

TABLE I.

Showing by Decennial Periods the Total Number of Deaths from Small-pox in Ireland, the Proportion of such Deaths to Deaths from All Causes, and the Annual Average Number of Deaths per 10,000 living, from 1831 to 1871.

1 DECENNIAL PERIODS	2 Deaths from Small-pox	3 Proportion of Deaths from Small-pox to Deaths from All Causes	ANNUAL AVERAGES	
			4 Deaths, per annum	5 Deaths per 10,000 living
1831 to 1841	58,006	1 in 20	5,801	7.095
1841 to 1851	38,275	1 in 36	3,827	5.842
1851 to 1861	12,727	1 in 64	1,273	2.195
1861 to 1871	4,113	1 in 187	411	0.759

These statistics show that deaths from small-pox have decreased in the decade 1861-71 to one-tenth what they were in the decade 1831-41. These facts might, of course, be accounted for by some on the supposition of some obscure epidemic influence, but they are accounted for in a most satisfactory manner by the progress of vaccination during these decades.

During the decade 1831-41 there was not any public provision for vaccination in Ireland, and, accordingly, small-pox had its full sway, as even those who desired to be vaccinated found it difficult to be so, unless they belonged to the wealthier classes of society. During this decade *one-twentieth* of all the deaths in Ireland reported to the Census Commissioners were caused by small-pox, and the average rate of mortality from small-pox was 7.095 in every 10,000 living. During the decade 1841-51 the first Vaccination

Act—namely, the 3rd and 4th Vic., cap. 29—came into force in Ireland, and public vaccinators were provided by the Poor Law Guardians; and thus vaccination was *offered*, but not made compulsory on the people. The result of affording these facilities was, that vaccination became more general and the average death-rate from small-pox fell from 7·095 to 5·842 per 10,000 living.

During the decade 1851–61 further facilities for vaccination were afforded by the passing of the Medical Charities Act, 14 and 15 Vic., cap. 68, by clause 13 of which every dispensary medical officer was “required to vaccinate all persons who came to him for that purpose.” Thus all excuse for neglect of vaccination was removed by the State—the result being that this provision was largely availed of, and the average death-rate from small-pox fell from a ratio of 5·842 to 2·195 per 10,000 living.

During the last decade, 1861–71, for which we have records, a most important change took place in the vaccination laws. On January 1st, 1864, during the prevalence of small-pox, the Compulsory Vaccination Act, 26 and 27 Vic., cap. 52, came into force, so that during seven years of this decade all persons born were bound to be vaccinated. The result of the enforcement of this Act is remarkable; for, during the decade, the mortality from small-pox fell to an average of 0·759 per 10,000 living—the total number of deaths returned to the Census Commissioners being but 4,113, of which 1,274 occurred before the Act came into force.

From the year 1864 we have more accurate means for judging of the relations between small-pox and vaccination. In that year the Act for the Registration of Births and Deaths in Ireland, 26 Vic., cap. 11, and the Compulsory Vaccination Act, 26 and 27 Vic., cap. 52, both came into force on the 1st of January.

The vaccination returns of the Irish Local Government Board, although not complete, are accurate, and include a very large proportion of the cases of vaccination in Ireland—namely, all those performed by the dispensary medical officers. Those performed by private practitioners or by the old Cow-pock Institution in Dublin are not included. The result of a comparison of the return of the Registrar-General and the vaccination returns of the Local Government Board is given in Table II.

TABLE II.

Showing the Number of Births in Ireland; the Number of Persons vaccinated by the Dispensary Medical Officers; the Number vaccinated who were born since the year 1864, and the Number vaccinated who were born before 1864; and the Number of Deaths from Small-pox for each year from 1864 to 1877, both years inclusive.

1 YEAR	2 Births in Ireland	PERSONS VACCINATED ^a			6 Deaths from Small-pox
		3 Total	4 Born since 1864	5 Born before 1864	
1864	136,414	191,810	— ^b	— ^b	854
1865	144,970	169,142	97,160	71,982	461
1866	146,090	137,124	104,780	32,394	194
1867	144,338	125,741	107,473	18,268	21
1868	146,051	131,426	118,613	12,813	23
1869	145,659	125,672	117,912	7,760	20
1870	149,846	140,220	135,057	5,163	32
1871	151,665	179,889	139,053	40,836	665
1872	149,292	282,484	142,662	139,822	3,248
1873	144,592	138,873	119,319	19,554	504
1874	141,411	139,587	119,337	20,250	569
1875	138,320	137,340	119,675	17,665	535
1876	140,438	114,487	112,489	1,998	21
1877	139,438 ^c	— ^c	— ^c	— ^c	61 ^d

Although Table II. does not contain a complete return of all vaccinations performed in Ireland, yet, inasmuch as the great bulk of the vaccinations in Ireland are performed by the dispensary medical officers, it affords an almost accurate measure of the relative number of vaccinations in each year. A comparison of columns 3 and 6 shows very remarkable results.

^a The vaccination returns are made up to the 30th of September in each year.

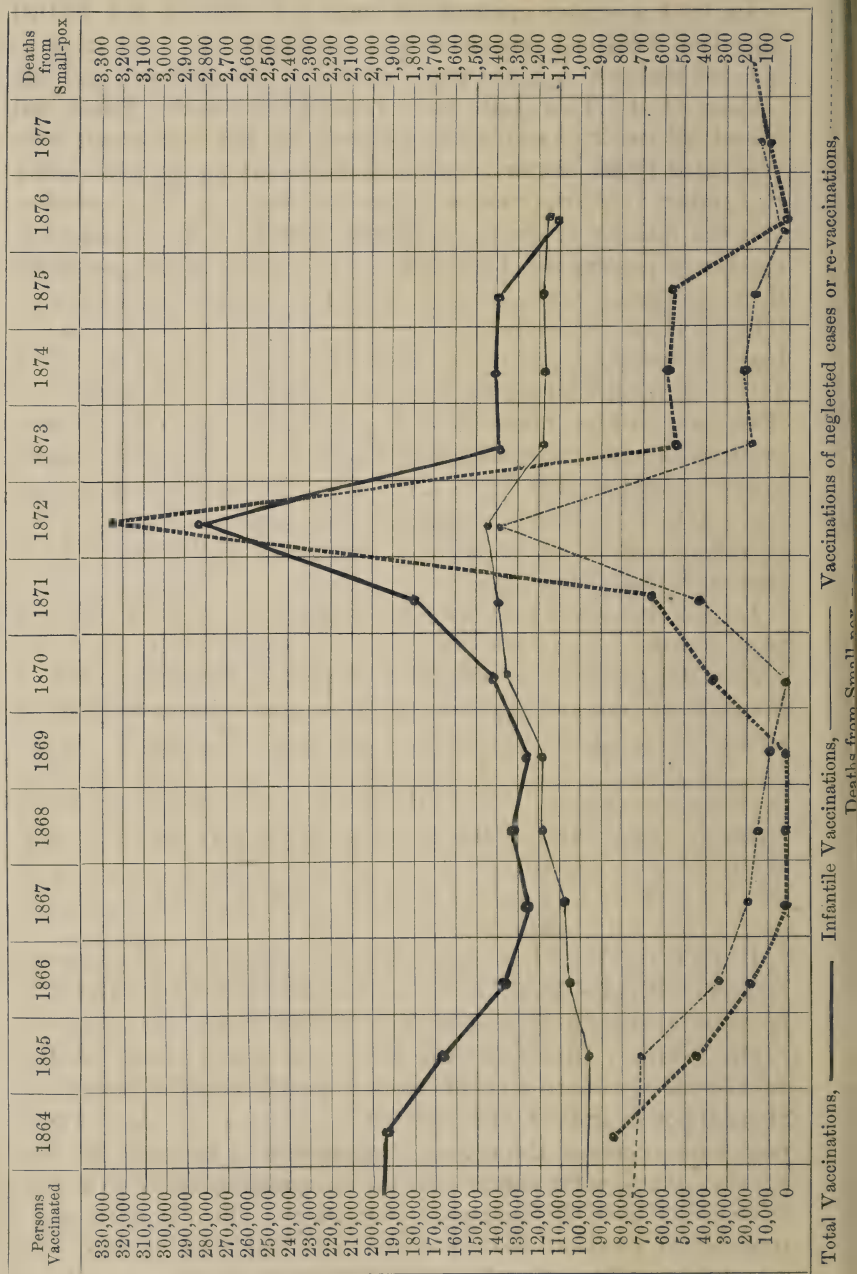
^b Not published, as the Acts were not in force during a portion of the year included in vaccination returns.

^c Returns applied for, but the Local Government Board "were not in a position to furnish them."

^d Kindly furnished by the Registrar-General for Ireland.

DIAGRAM I.—Showing the Relation between the Prevalence of Small-pox and Vaccination in Ireland.

(SEE TABLE II.)



Total Vaccinations, — Infantile Vaccinations, — Deaths from Small-pox, Vaccinations of neglected cases or re-vaccinations,

In 1864 a serious epidemic of small-pox (causing 854 deaths) prevailed, which had commenced in the previous year. The enforcement of the new Act, the panic caused by the epidemic, and the circular of the Poor Law (now Local Government) Board had stimulated vaccination to such an extent that 191,810 persons were vaccinated by the dispensary medical officers during the year ending September, 1864, the number of vaccinations exceeding considerably the number of births registered, showing that a considerable number of persons had been either re-vaccinated or had postponed their vaccination until forced to it by the dread of the small-pox. The result of this activity in vaccination was a diminished prevalence of small-pox, the deaths from this disease falling to 461 in 1865, and further falling the next year, when the epidemic ceased. The panic having now subsided, and the primary effect of the enforcement of the new Act having passed away, vaccination became neglected, and steadily fell to 125,672 in 1869, and but few persons who had been neglected or required re-vaccination submitted to the operation, only 7,760 being recorded, against 71,982 in 1864-65. (See column 5, Table II.)

On March 7, 1865, the Poor Law Commissioners issued a circular to all boards of guardians calling attention to the fact that many persons did not comply with the Compulsory Vaccination Act of the previous year, and impressing upon the guardians the necessity of strictly enforcing the provisions of the Act. This seems to have had the desired effect, for the number of infantile vaccinations increased considerably, and continued to increase during the three following years. During this period, as might have been expected, the number of vaccinations of persons not within the operation of the Act declined to a marked extent, so that the total number of vaccinations fell considerably.

In 1866 the Poor Law Board did not consider it necessary to issue any circular about vaccination, rightly believing that their previous circulars were producing the desired effect.

On February 6th, 1868, the Poor Law Board issued another circular calling the attention of boards of guardians to the diminution of small-pox in Ireland, and calling for returns of any cases which may have occurred throughout the country. In reply to this circular, the Commissioners received information relative to the existence of cases of small-pox in five unions. Two of these were traceable to England, and the other three had been produced by inoculation. The Board issued a further circular on 7th of

March, 1868, calling the attention of the guardians to these cases, and remarking—"Much as the guardians may be disposed to congratulate themselves upon this apparent approach to the extinction of small-pox in Ireland, the Commissioners feel bound to warn them that the continuance of immunity from this epidemic must depend upon continued activity in carrying out the provisions of the Compulsory Vaccination Act, and that even greater exertions than any yet made ought to be made to secure that end, inasmuch as there are many districts in which vaccination is not so thoroughly carried out as it should be, and some in which it has been very culpably neglected." This was most important advice and reasoning to those having the local administration of the vaccination laws. The advice seems, however, to have had but little effect, as infantile vaccinations diminished by 701, and vaccinations of neglected or previously vaccinated cases by 5,053, in the succeeding year. On September 20th, 1869, the Poor Law Commissioners again issued a circular announcing the cessation of small-pox in Ireland, and again warning the local vaccination authorities in the following words:—"Whether or not small-pox remains dormant in the country, and may be expected again to break out under less favourable circumstances, it is quite clear that every part of Ireland is at all times exposed to the introduction of the disease, either accidentally or by design, and, therefore, that the only security lies in a steady maintenance everywhere of the protective means afforded by vaccination." On the 21st of December, 1869, the Poor Law Board issued a circular calling attention to the death of a Swedish sailor in the Hardwicke Hospital from small-pox, and to the occurrence of a case at Maynooth, arising either from this case or from a girl from the Royal Barracks who was in the hospital at this time, and also to a case in Belfast introduced from Liverpool. The circular concludes with another warning about the danger of neglected vaccination. This warning, together with the alarm created by the actual presence of small-pox, had a better effect than the previous circulars, and infantile vaccinations increased, but re-vaccinations continued to decline. Just about this time, on Nov. 18th, 1869, Dr. Burke, Superintendent of Statistics at the General Register Office, now Registrar-General for Ireland, was examined before the Royal Sanitary Commission. In reply to the question (8841), "Are you satisfied that the singular diminution in the amount of small-pox is owing to compulsory vaccination?" Dr. Burke said, "The diminution is very remarkable, but no

epidemic of small-pox has occurred since the introduction of the Compulsory Vaccination Act." Again, answering the question (8842), "I presume that you do not go so far as to say that, owing to compulsory vaccination, small-pox has been stamped out in Ireland, or that it will be stamped out?" Dr. Burke said, "I should be very sorry to make use of such an expression. I think you should have the test of an epidemic in the country before you would use such strong terms." The test came very soon, and found how far small-pox was from being stamped out in Ireland. If others had been as far-seeing as Dr. Burke, and if accurate and complete vaccination returns had been frequently published and compared with the births, as they should have been, the susceptibility of the population of Ireland to small-pox could have been as easily and more safely gauged than by the introduction of small-pox contagion. As I have pointed out, the circular of the Poor Law Commissioners, issued on the 21st of December, 1869, produced some results which were noted in the published returns for the year ended September, 1870; but the activity came too late, for small-pox had already sown its seed in Ireland, and a most destructive epidemic followed, which lasted for five years. The Poor Law Commissioners continued to issue warnings to the local authorities during the year 1870, and on December 15th announced a serious outbreak of the disease in Belfast. On February 7th, and again on March 2nd, 1871, the Commissioners called the attention of the local authorities to the importance of re-vaccination; and on July 13th, 1871, announced the appearance of the disease in 53 unions of Ireland. This, together with the increased prevalence of small-pox, had the desired effect, and at last, in 1872, vaccination and re-vaccination became sufficiently effectual to check the epidemic. In fact, the vaccinations by the dispensary medical officers in Ireland rose to the unprecedented number of 282,484, the births numbering but 149,292. Small-pox at once declined in all except the remoter parts of the country, where it continued its progress for some time longer. In the Dublin district public vaccinations reached the immense number of 19,069, the births being only 8,499, in 1872. This complete vaccination and re-vaccination of the whole community seems to have stamped out the epidemic. On looking at the tables, it is evident that it was the increase of re-vaccinations and the vaccination of neglected cases that stopped the career of the epidemic. A glance at the Tables II. and III. will show that as soon as vaccination fell very low—namely, in

1869—small-pox began to make its appearance and grew into the terrible epidemic of 1872. Another look at the Tables will show that in 1876 vaccination had fallen lower than it had in 1869, and immediately the present epidemic made its appearance.

In 1872 the Poor Law Board was converted into the Local Government Board, and all the sanitary powers in the country placed under its control. From this time forward it does not appear that the newly-constituted board issued any circulars to the board of guardians relative to the imperfections in the administration of the vaccination laws, although the annual reports of the Board refer to the neglect of infantile vaccinations. In the report for 1876-77 important circulars relative to lymph-supply are contained, and a long correspondence with the English authorities relative to the possible introduction of small-pox into Ireland through the agency of deported paupers. The omission to warn the local authorities in Ireland of the prevalence of small-pox in the towns in the north of England in daily communication with Irish ports, and of the appearance of small-pox in Dublin, seems to be a most unaccountable oversight.

I cannot but think that had a circular been issued noticing a recurrence of the same circumstances that initiated the epidemic of 1871-73, and calling attention to the diminution of vaccination which had occurred in the year 1875-76, that the present epidemic might in some measure have been retarded and mitigated.

It is not necessary to give in detail the relation of small-pox in Dublin to the enforcement of the Compulsory Vaccination Act and the circulars of the Poor Law and Local Government Boards, the fluctuations following them being already noticed for the whole of Ireland. The details with regard to Dublin are given in Table III. For the information in this Table I am indebted to the reports of the Local Government Board and the Registrar-General, together with special information presented by Dr. Sinclair, the able Secretary of the newly-established Vaccine Department of the Local Government Board. Dr. Sinclair not only kindly placed the statistics of his department at my disposal, but also himself assisted me in extracting the required information from the very voluminous and accurate books of the old Cow-pock Institution and the present Vaccine Department. The total of the figures presented through Dr. Sinclair's kindness are given in column 4. I have also to thank the Sisters of Mercy of the Mater Misericordiæ Hospital and the Registrar of Sir Patrick Dun's Hospital for

furnishing me with the number of small-pox cases admitted to these hospitals during the last 14 years, and the Registrars of the Hardwicke, Meath, Steevens', and Cork-street Hospitals for supplementing the returns of the Board of Superintendence. The Registrar-General also kindly furnished, in advance, the number of births, and deaths from small-pox, in Dublin and Ireland for the last year. I regret that the Local Government Board declined to furnish me with copies of the unpublished return of vaccinations performed in the Dublin Registration district by the dispensary medical officers during the last year.

I find that upwards of 20 per cent. of all the public vaccinations in the Dublin district are performed at the Vaccine Department of the Local Government Board and its branches.

TABLE III.

Showing the Number of Births Registered; the Number of Vaccinations performed by the Dispensary Medical Officers, and at the Cow-pock Institution; the Total Number of Public Vaccinations in the Dublin Registration District; the Number of Cases of Small-pox Admitted to the Dublin Hospitals; and the Number of Deaths from Small-pox Registered in the Dublin Registration District from the year 1864 to the year 1877, both years inclusive.

1 Year	2 Births registered ^a	PUBLIC VACCINATIONS			6 Small-pox cases admitted to hospital ^c	7 Deaths from Small-pox ^a
		3 Vaccinations at Dispensaries ^b	4 Vaccinations at Cow-pock Institution ^b	5 Total ^b		
1864	8,753	7,572	1,784	9,356	31	42
1865	8,903	7,408	1,473	8,881	212	71
1866	8,695	4,752	1,218	8,970	139	25
1867	8,241	4,889	1,217	6,106	70	2
1868	8,846	4,236	1,266	5,502	1	1
1869	8,276	4,718	1,402	6,120	1	1
1870	8,546	4,886	1,273	6,159	89	0
1871	8,860	7,630	2,071	9,701	1,932	207
1872	8,499	15,297	3,772	19,069	652	1,350
1873	9,032	4,109	1,260	5,369	4	16
1874	8,903	4,270	1,353	5,623	3	2
1875	8,680	5,038	1,421	6,459	41	2
1876	9,006	4,266	1,416	5,682	110	9
1877	9,325	—	1,824	—	387	38

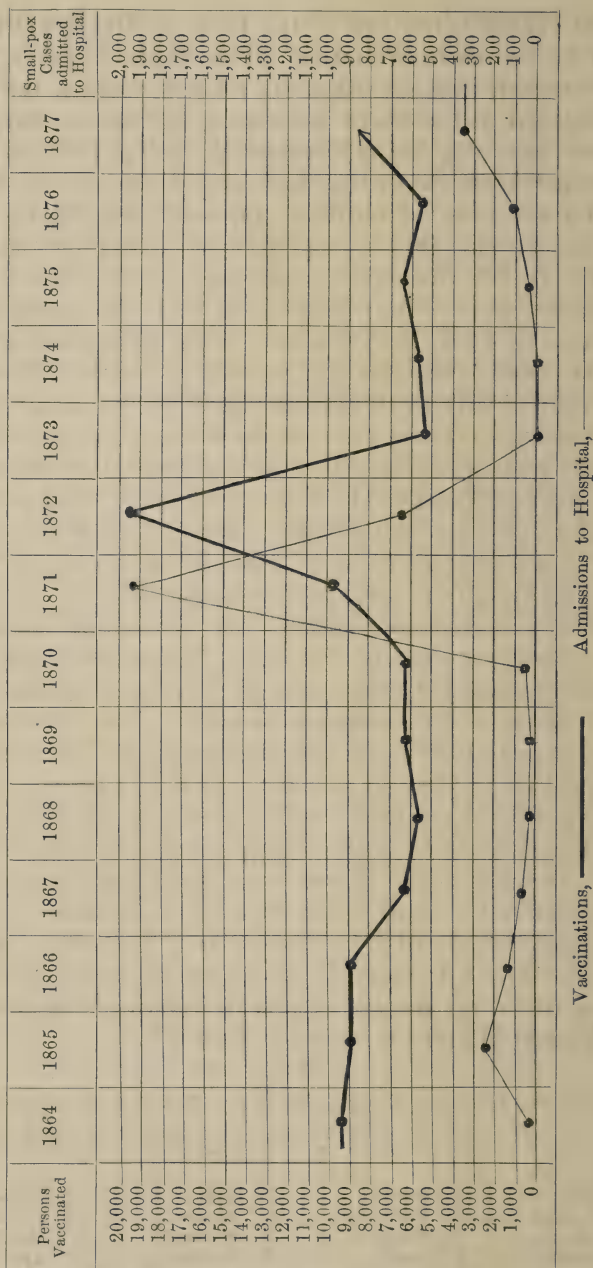
^a Year ends December 31st.

^b Year ends September 30th.

^c Nearly all the hospital years are made up to 31st of March, which accounts for the discrepancy between column 6 and column 7.

DIAGRAM II.—Showing the Relation between the Prevalence of Small-pox and Vaccination in Dublin.

(SEE TABLE III.)



The general indications of the Table for Dublin are the same as those for the whole of Ireland. This Table, however, points out more strongly than the other the relation between the prevalence of small-pox and deficient vaccination on the one hand, and the power vaccination has of overcoming small-pox on the other. In 1868 and 1869 vaccination had become very low in Dublin; in 1870 a few cases of small-pox appeared, and slightly stimulated vaccination; in 1871 a considerable number of admissions to hospital further stimulated vaccination, but it was not until *after* the disease had become widespread in 1871 that vaccination became excessive (if I may use the term). Thus small-pox had reached its height before vaccination had reached *its* height. Now, to come more particularly to the present epidemic, we find that in 1876 small-pox became very active in the towns in the north of England in daily communication with Dublin and the other seaports on the east coast of Ireland. In July of that year small-pox was introduced into Dublin by the usual channels. A woman from Manchester was admitted to Sir P. Dun's Hospital on July 27th, 1876, while suffering from small-pox, and communicated the infection to another patient, who developed the disease in Clarendon-street, which became a centre of contagion. In another instance, a servant from Stonyhurst College, visiting Liverpool, became ill in that town, and immediately started for Dublin, where, on his arrival on September 13th, 1876, he was admitted to the Mater Misericordiæ Hospital for an affection believed to be fever; when in the hospital, small-pox developed itself; and, by a series of unfortunate accidents, the disease in the hospital was propagated outside from this source. Thus two foci of the disease were established—one on the north, the other on the south side of the city—both having originated by contagion introduced from Lancashire. The disease having been thus disseminated, gradually spread, and its progress may be easily measured by the number of patients suffering from it admitted to Cork-street Hospital as shown in Table IV.

TABLE IV.

Showing the Number of Cases of Small-pox Admitted per Month to Cork-street Hospital since the commencement of the present Epidemic.

1876.		1877.		1877.		1878.	
August . .	1	January . .	8	July . .	2	January . .	19
September . .	0	February . .	5	August . .	4	February . .	76
October . .	0	March . .	7	September . .	2	March . .	106
November . .	3	April . .	3	October . .	3	April . .	173
December . .	5	May . .	4	November . .	8		
		June . .	3	December . .	23		

Comparing this with Table V., which shows the number of admissions per month during the epidemic, 1871-73, it appears that the present epidemic was slower in reaching formidable proportions, sixteen months having elapsed before the monthly admissions exceeded 8. In the previous epidemic but eight months elapsed before a similar period of intensity was reached. It is an important question how much of the retardation of the present epidemic may be due to the destruction of infected clothing, from August, 1876, to March, 1877. I believe it has had an important influence, and am glad to say the Public Health Committee have returned to the wise and economical course of destroying infected clothing. Measuring the prospects of the present epidemic by what occurred in the last, we should be prepared to deal with a considerable number of cases during the next six months. It is to be hoped, however, that increased activity in vaccination and re-vaccination, with special sanitary precautions, may cut short the epidemic.

TABLE V.

Showing the Number of Cases of Small-pox Admitted per Month to Cork-street Hospital during the Small-pox Epidemic of 1871-73.

1871.	1871.	1872.	1872.
February . 1	September . 8	March . . 97	October . . 7
March . . 1	October . 41	April . . 65	November . 3
April . . 7	November . 59	May . . 59	December . 8
May . . 5	December . 78	June . . 64	1873.
June . . 7	1872.	July . . 34	January . . 2
July . . 1	January . 81	August . . 16	February . 1
August . . 4	February . 90	September, . 7	March . . 0

My personal experience of the present epidemic has been almost altogether derived from Cork-street Hospital, but having retired from active duty at that institution on the 31st of March, I must look to my successors for information relative to any peculiarities which may arise in future.

Up to the last day of March the records of Cork-street Hospital show that, since the commencement of the epidemic in the autumn of 1876, 295 cases were admitted. These are classified in Table VI. The term malignant is used to designate cases of purpuric small-pox accompanied by hæmorrhage.

TABLE VI.

From April 1st, 1876, to March 31st, 1878.

Varieties	Number of Cases	Proportion per cent. of all the cases	Recovered	Died	Mortality per cent.
Discrete - - -	131	44·0	129	2	1·5
Confluent - - -	148	50·3	116	32	21·6
Malignant - - -	16	5·7	3	13	81·2
Total - - -	295	100·0	248	47	15·8

From these statements it appears that the per-centage of each variety was—Discrete, 44·0; confluent, 50·3; and malignant, 5·7, against 61·8, 31·7, and 6·5, respectively, in the epidemic of 1871–73. The mortality has also been lower than in the last epidemic, when the total per-centage mortality was 21·6, against 15·8, shown above. Not only has the total rate of mortality been lower, but the mortality in each class has been less. The mortality among the confluent cases showed a marked reduction from 47·6 to 21·6. This is easily accounted for by the fact that of the confluent cases in 1871–73, only 60·4 per cent. were vaccinated, whereas in the present epidemic 70·9 were vaccinated (see Table VII.). In the discrete variety the mortality was, as usual, insignificant, one having died of consumption, the other of meningitis, and therefore died *in*, but *not of*, small-pox.

TABLE VII.

From April 1st, 1876, to March 31st, 1878.

	DISCRETE			CONFLUENT			MALIGNANT			TOTAL		
	Total	Died	Per cent. Mortality	Total	Died	Per cent. Mortality	Total	Died	Per cent. Mortality	Total	Died	Per cent. Mortality
Vaccinated -	122	2	1·6	105	10	9·4	6	4	66·6	233	16	6·8
Unvaccinated	9	—	—	43	22	51·1	10	9	90·0	62	31	50·0
Total -	131	2	1·5	148	32	21·6	16	13	81·2	295	47	15·8
Per cent. vaccinated in each class	93·1			70·9			37·5			78·9		

This Table shows that of discrete cases 93·1 per cent. were vaccinated, and of these only 2 died, or at the rate of 1·5 per cent., neither of whom died of small-pox. There is a marked contrast among the confluent cases, of whom only 70 per cent. were vaccinated; of these 32 died, or at the rate of 21·6 per cent., the rate of mortality of the vaccinated being only 9·4 per cent., while among the unvaccinated it was 51·1 per cent. Among the malignant cases we find that the mortality among the vaccinated was only 66·6 per cent. against 90 per cent. in the unvaccinated. Adding all classes together, we find that the mortality among the vaccinated was only 6·8 per cent. against 50 per cent. among the unvaccinated, proving how much the disease is mitigated by vaccination.

Up to the present there has not been any case admitted to Cork-street Hospital who has been re-vaccinated.

I think it may be interesting to present, in a combined form, the information I have collected at Cork-street Hospital during the last and present epidemics. This I have done in Tables VIII. and IX., in which the particulars of upwards of 1,000 cases of small-pox are tabulated. Considerably over one-half of these cases came under my immediate care, and I can vouch for the accuracy of the statements with regard to those which were treated by my colleagues.

TABLE VIII.

Statistics of Small-pox in Cork-street Hospital.

Varieties	Number of Cases	Proportion per cent. of all the cases	Recovered	Died	Mortality per cent.
Discrete - - -	591	56·8	582	9	1·5
Confluent - - -	385	37·0	240	145	37·7
Malignant - - -	65	6·2	10	55	84·6
Total - - -	1,041	100·0	832	209	20·1

It will thus be seen that of 1,041 cases, 591 were discrete, 385 confluent, and 65 malignant, or in the relative proportions of 56·8, 37·0, and 6·2 per cent. Of the 591 discrete cases, only 9 died, or at the rate of 1·5 per cent. Of the 385 confluent cases, the mortality was 37·7 per cent. Of the 65 malignant (purpuric and hæmorrhagic) cases, 55 died, or at the rate of 84·6 per cent.

TABLE IX.—*Statistics of Small-pox in Cork-street Hospital.*

	DISCRETE			CONFLUENT			MALIGNANT			TOTAL		
	Total	Died	Per cent. Mortality	Total	Died	Per cent. Mortality	Total	Died	Per cent. Mortality	Total	Died	Per cent. Mortality
Vaccinated -	565	3	0·5	248	56	22·6	31	22	71·0	844	81	9·7
Unvaccinated	26	6	23·1	147	89	65·0	34	33	97·1	197	128	64·9
Total -	591	9	1·7	385	145	37·7	65	55	84·6	1,041	209	20·1
Per cent. vaccinated in each class	93·6			64·4			47·7			81·1		

Table IX. proves in as striking a manner as Table VII. the great advantage the vaccinated have over the unvaccinated patients. It will be seen that in each variety the mortality of the vaccinated was much less than of the unvaccinated. In the discrete it was 0·5 per cent. in the vaccinated against 23·1 per cent. in the unvaccinated; in the confluent, 22·6 per cent. against 65 per cent.; and in the malignant variety, 71·0 per cent. against 97·1 per cent. The total result shows that among these 1,041 cases the rate of mortality among the vaccinated was but 9·7 per cent., while among the unvaccinated it was 64·9 per cent.

TABLE X.

Ages				Total	Per cent. at each Age	Vaccinated	Per cent. Vaccinated	Died	Mortality per cent.
Under 5	-	-	-	24	8·0	10	41·6	12	50·0
5 and under 10	-	-	-	38	13·2	27	71·0	3	7·8
10	„	15	-	36	12·5	29	80·4	6	16·6
15	„	20	-	58	19·6	52	89·6	2	3·4
20	„	30	-	97	32·8	79	81·4	15	15·4
30	„	40	-	27	9·0	22	81·5	7	26·0
40	„	50	-	11	3·6	10	90·9	2	18·1
50 and upwards	-	-	-	4	1·3	4	100·0	—	—
Total	-	-	-	295	100·0	233	75·5	47	15·8

Table X. shows the ages of small-pox patients admitted to Cork-street Hospital during the present epidemic.

It will be observed the largest number were young adults between 15 and 30 years of age. This table is also to a great extent confirmatory of the conclusion drawn from Table III. relative to the decrease of vaccination in Dublin. Thus, of children admitted under 5 years only 41·6 per cent. were vaccinated; of those from 5 to 10 the vaccinated cases were 71 per cent.; and from 10 to 15 the vaccinated cases reached 80·4 per cent. It is a sad fact that of the cases admitted during the present epidemic 32 were unvaccinated, although within the provisions of the Act of 1864.

I have now given a general account of the epidemic as it has at present come under my notice. I shall now consider what measures should be taken to avoid such visitations in future, and if possible to limit the extent of the present epidemic.

I have pointed out that the chief promoting cause of the present, as it has been of past small-pox epidemics, is the neglect of vaccination. The existing vaccination laws are loosely carried out—the local authorities abstaining from prosecuting parents for neglecting to have their children vaccinated or to bring them for inspection on the eighth day; and the magistrates being unwilling to inflict penalties on persons who have violated the law. When these faults are complained of, the general rejoinder given by the boards of guardians and the magistrates is—"Oh, the law is deficient. Give us an efficient law and we will administer it efficiently." Although this rejoinder is more of an excuse than a reply, yet it must be admitted that the vaccination laws of Ireland are in very many important particulars deficient, and the deficiencies are of a nature which do not exist in the laws at present in force in England.

These defects are as follows:—

(a.) A child may remain unvaccinated for a period of six months after its birth without its parents incurring any penalty. The period in England is but three months.

(b.) Public vaccinators in Ireland have no power to take lymph as in England, thus limiting considerably the lymph-supply.

(c.) There is no inspection of vaccination in Ireland.

(d.) The payments to public vaccinators in Ireland are so small that it is not worth their while to be zealous in the discharge of their duty.

More than a year ago a bill to remedy all these defects was

drafted by the Council of the Irish Medical Association, and presented to the Government, as also a bill for consolidating and amending the vaccination laws in Ireland. No step has yet been taken to bring either of these bills before Parliament, although it is now several years since the vaccination laws in England were amended, and the medical corporations in Ireland have impressed upon the Government the importance of action in the matter. So far as the prevention of the present epidemic is concerned, legislation can have little influence, and hundreds of valuable lives will be lost before any new vaccination law can be passed. I cannot consider the defects in the law as a sufficient excuse for the culpable neglect of the boards of guardians to enforce the existing laws, which at all events give power, under penalty, to compel vaccination within six months of birth, and require parents to bring their children for inspection on the eighth day after vaccination, so that the medical officers may be able to judge of the success of the operation. Before leaving the question of vaccination there is another point to which I wish to refer—namely, the absence of any complete published return of vaccination in Ireland. It is impossible at present to ascertain the extent to which vaccination has been performed in Ireland. In England complete returns are published by the Local Government Board, but only once a year. In Scotland returns are published quarterly by the Registrar-General. In Ireland the returns published by the Local Government Board are only of the successful vaccinations performed by the dispensary medical officers and the workhouse medical officers. I believe myself that the Scotch method is the best, inasmuch as it brings the state of vaccination more frequently before the public, and places it in conjunction with the returns of births and deaths. I believe, if this question were brought under the notice of the Government, that a remedy for the present defective returns would be provided either by the Registrar-General or by the Local Government Board.

Besides neglected vaccination there are unfortunately other promoting causes of small-pox permitted to exist in Dublin, which do not require any new enactment for their removal:—

1. The disgraceful sanitary state of the city, as evinced by its high death-rate.
2. The want of proper sanitary organisation and supervision.
3. The want of proper means of removing infected persons to hospital.

4. The want of epidemic hospital accommodation. This is a great defect in hospital organisation in Dublin. There is, in fact, but one epidemic hospital in Dublin, and that not sufficiently large to cope with such an epidemic as the present. It is utterly contrary to all sanitary knowledge to receive patients suffering from such a disease as small-pox into the body of a general hospital building—as, I regret to say, has been done, in some instances with disastrous results. The expedient of providing temporary accommodation in each epidemic when it has spread to a sufficient extent to alarm the public mind, seems adopted upon the utopian idea that each epidemic will be the last. The present epidemic has brought with it the usual scramble at our hospitals, the usual wait-till-the-last-minute policy at the hands of guardians, and will not terminate until either each person has taken steps for his own safety or every susceptible person has been attacked.

Although this epidemic has been a year progressing in Dublin, although the Dublin Sanitary Association, more than a year ago, warned the authorities of the impending danger, yet not a move was made—except by the Sanitary Association—until three months ago—to provide for the sick, or stay the progress of the epidemic, under the foolish belief that efforts of this sort would produce “unnecessary alarm,” and with the comforting assurance that small-pox had not “assumed an epidemic character.”

ART. XVII.—*Clinical Report on Epidemic Measles in Kells, with some Remarks on the Principal Sanitary Measures useful in this Disease.* By RICHARD J. HALTON, L.K.Q.C.P.I.; L.R.C.P.E.; L.R.C.S.I.; Mem. Obs. Soc.; Med. Officer to the Dispensary; and formerly Medical Officer of Health to Kells.

THE widespread prevalence of measles, during the opening months of the present year, and the fatal character of its associated chest affections in many parts of Ireland, have naturally excited considerable attention. I have, therefore, deemed it not inopportune to detail shortly its progress and results in this overcrowded town, where it has shown itself with some virulence, and where it has spread with a rapidity altogether novel in my experience of the district, more than two hundred cases having occurred within two months of its first appearance.

The type of the disease was, in most respects, normal. Coryza

and cough followed by papular, slightly elevated dusky red rash appearing first on the face and forehead, and spreading next to the arms and chest, and then to the thighs and legs, where it was usually thicker, heavier, and more elevated than on other parts of the body. One point of difference from most classical descriptions of the disease was noted. This was the entire absence of any sign of illness by which the stage of incubation could be detected. In the vast majority of cases, until the coryza, cough, or vomiting appeared—and those in the order named were the most frequent premonitory symptoms—the closest inquiry failed to elicit the smallest evidence of ill health, or even languor. The rash, also, often appeared on the day or the day but one after the first symptom of illness. In contra-distinction to this there were a few cases where the stage of incubation was both marked and prolonged, leading to considerable doubt as to the nature of the illness. In one case a kind of erysipelatous, scruffy blush behind the ear preceded the appearance of the rash, while, in many of the cases, the point of the chin was the starting place. During the first two or three hours a red patch in this situation, looking much more like erysipelas than measles—perhaps from being rubbed or scratched—was the only sign of eruption on the body. Diarrhœa was common at the subsidence of the rash, but it occurred in one case as a premonitory symptom, and in another case a profuse perspiration preceded the eruption.

The almost universal habit of dosing the children with hot punch or burnt whiskey to bring out the rash thickly, seemed in some cases to cause a certain amount of cerebral congestion, but, with one exception, it was attended with no serious results. In the case alluded to the boy, about eight or nine years old, heavy, fat, and thick-necked, began to rave, fell into a comatose state on the third or fourth day of his illness, and sank rapidly, dying within twenty-four hours. In only one other case did cerebral symptoms show themselves, and they were, in this instance, the precursors of a mild attack of measles. I extract the following abridgment from my note-book:—

March 3rd.—D. S., aged twenty, was quite well until a day or two ago, when she began to complain of pains in her bones and some joints, particularly the wrist, on which account she stayed in bed. To-day she rather suddenly began to rave, which alarmed her friends, and induced them to send for me. I found her talking nonsense, wanting to know where she was, and making frequent attempts to get out of bed, in which, however, a little firmness sufficed to keep her. Pulse 100, soft and

equable, and temperature but one degree above normal; eyes very sensitive to light; some headache. Her mother, an exceedingly intelligent well-informed woman, is positive she had the measles some years since. The delirium yielded to mild measures—a mercurial purge, a diaphoretic mixture, with aconite, and cutting off the back hair. A day or two afterwards a severe fit of retching was succeeded by a short cough, and on March 13th a copious eruption of measles took place. There is little more to note, as she was able to be out by the 23rd, though there remained slight red traces on her face and chin, and her eyes were still painful and troublesome.

The principal source of danger, however, as in most measles epidemics, arose from the aggravation of the catarrhal symptoms ensuing on the decline of the rash, and which, in a large proportion of the cases, rapidly passed into capillary bronchitis or pneumonia. To those affections, with the exceptions already recorded, all our fatal cases were due. These cases ran a rather rapid course, few surviving the fifth or sixth day from the commencement of the rapid breathing. Differential diagnosis was not attained in the majority of the cases, the subcrepitant râle being the sound commonly audible, while the dulness on percussion over lung bases, with two or three exceptions, was neither distinct nor limited. The breath rate was in general very high, rising to 80 and over in the minute, the alæ nasi in rapid motion, and the cough, severe and frequent at first, but gradually subsiding as the termination approached. The intelligence was usually unimpaired to the last, and death seemed the result of simple exhaustion. There was a notable absence of flushing, lividity of the countenance occurring early, with unusual brilliancy of the eyes. The following case, as an example of very rapid breathing, may be shortly cited:—

March 20th.—C. S., aged four months. This, a remarkably healthy and large child when born, got what the mother calls a general, red rash when two months old. She has now had the measles which attacked her as the first rash was subsiding. She got a cough after the measles, which rapidly became worse. She has now hardly the power to swallow, and when she attempts it, it brings on a short convulsive cough with suffocative sound. Tongue is curdy white, and palate also covered with the same coating, which ceases, however, before the fauces are reached. The gums and inside of cheeks are exsanguine. She lies on her back with her head somewhat retracted, and breathing 114 times in a minute. It was not convenient to practise auscultation. She died eight hours after my visit.

The treatment pursued consisted of sinapisms and warm poultices of linseed meal to the chest, expectorants and diaphoretics internally, with mild mercurial aperients when necessary. In the case of infants at the breast, a plentiful supply of cold water given simply by spoonfuls, was eagerly taken, and found very serviceable in allaying irritability. It was also observed that well salted, thin chicken broth was taken much more readily than milk, and agreed much better during the height of the disease, convalescence being marked by the return of the taste for milk. Wine was given liberally where the strength began to fail, and in some cases with markedly beneficial results. I cite one in illustration:—

March 23rd.—D. B., aged nine, after a prolonged incubative or premonitory stage—the cough, coryza, and occasional vomiting lasting nearly a month—the eruption of measles appeared to-day. He does not feel sick, takes his food, and jumps out of bed to play about the room, if not watched.

March 24th.—Got up this morning to have the bed made, and fainted when he got on the floor. Had some bilious vomiting shortly after, and his breathing was noticed to be very hurried. Thick eruption close together in large patches. Respirations 80 per minute; pulse 132; occasional hiccup which he complains hurts him; constantly restless and moaning; subcrepitant râles over both bases; expectorant and diaphoretic mixture at short intervals; dry cupping over bases, and poultices.

March 27th, 9 a.m.—Bad night; slept none; attempts to vomit, but brings nothing up; very restless; coughs once, occasionally it sounds soft; crepitation over bases a little more audible; breath rate not noted, but rapid; pulse 148, failing occasionally; temperature 105° ; complains of faintness and of being very tired; also of great heat in hands and feet, which was relieved by tepid sponging. A dessert spoonful of champagne every hour; a teaspoonful or two of chicken broth every half hour; five grains of carbonate of ammonia in effervescence every two hours. 7 p.m.—Sickness of stomach disappeared; takes his stimulants and nourishment well; pulse 132; temperature 103.2° ; much calmer; to lessen the champagne.

March 28th.—Had a better night; much quieter; pulse 124; temperature 100° ; but cough is now frequent and troublesome; to stop the champagne, and resume poultices and expectorants.

March 29th.—Good night; still coughing, but asks for food; he gradually recovered.

There is only one further point about treatment on which I would lay stress; but I consider it an extremely important one in the acute respiratory diseases of infancy and childhood. I allude to the

method of administering expectorant remedies. I will suppose, for instance, that the following mixture is ordered for a child from five to seven or eight years old, in the early stage of acute bronchitis:—

Ipecacuanha wine,
 Spirit of nitrous ether, of each ʒss .
 Water of the acetate of ammonia, ʒi .
 Water, to ʒiv . M.

To which may be often added with advantage:—

Iodide of potassium, grs. 16.
 Tincture of aconite, min. xv.

The directions will usually be two teaspoonfuls every three or, perhaps, four hours. If the effect of this dose is watched it will be apparent that soon after its administration, say within five minutes, the cough has become softer and the breathing is easier; but when a quarter of an hour or twenty minutes has elapsed, this effect has passed away, and the cough is again hard and the breathing fast. It is too soon to repeat the medicine, as this would probably cause vomiting, which may not be suitable under the circumstances. The alternative is to give smaller doses, but repeat them much more frequently. Half a teaspoonful every half hour, or, in bad cases, half that dose diluted with a little water every quarter hour, will give relief every time, and will, in the end, produce much more effect than the larger dose repeated at the longer interval. The same rule also applies to external applications. Clinical experience amply proves that children, both as to their skin and secretions, are much more quickly and readily affected than adults, but that the effect is proportionately evanescent. A hot poultice will make the skin act in ten minutes, and should be removed after a further similar interval, to be re-applied in an hour or an hour and a half, as the case may be. If left on after the first effect for good has been produced, they soon cease to afford relief, and become rather a source of distress and discomfort to the little patient wearily panting for breath. Sinapisms, also—which seem, when frequently applied, shifting their position each time, to be in some cases more useful than poulticing—should be left on until the first sensation of heat is experienced, if the child is old enough to express its feeling. Five minutes longer then will be quite sufficient; but if the breathing is up to 80, and the case, consequently, a dangerous one, they should be re-applied every half hour until some relief is obtained. This mode of treatment is, no doubt,

troublesome, and requires close and constant watching; but in even desperate emergencies it will be found amply to repay the trouble.

In considering the sanitary aspects of the epidemic, some very interesting questions present themselves. There has been no epidemic of measles in Kells for the past eight years, and only very few isolated cases. In the spring of 1874, some ten or fifteen cases occurred in a little village about three miles from Kells, none of them terminating fatally. At this period one single case occurred in Kells, presumably receiving the infection from that source. Why did not the disease spread then, though introduced from a centre of infection so close to us, and with which we were in constant communication; and why did it spread so rapidly now, when it must have come from a much greater distance, for the district was absolutely free from the disease, or a suspicion of it, when the first case occurred in the very centre of the town? Epidemic wave or epidemic atmosphere is an unsatisfactory answer; but the rapid nature of the spread would seem to admit of no other explanation. The first case occurred on January 4th. In a little more than a week there were ten cases discovered in different and widely distant parts of the town, all of them stricken since that date, and in various stages of progress. Before a month I had fifty cases on my visiting list. It is apparent that isolation or hospital treatment, under circumstances like these, is quite impracticable. That this will be always the case I think not improbable. The disease itself, however, is rarely dangerous, and the subsequent chest affections being the great source of peril, it follows from this, that while sanitary science instructs us to prepare *beforehand* for all the other eruptive fevers, and that it is often quite practicable to treat them by the method of exclusion, we can combat the direful effects of measles with success *after* the invasion of the disease. Heat, artificial warmth, is the principal sanitary measure requisite. Of course, the subsidiary aid given by pure air, cleanliness, and proper nourishment is always, when practicable, to be availed of. All the fatal cases, with the single exception of the infant's case already quoted, occurred in cabins, mostly single-roomed, with crazy doors, broken windows, and innumerable crevices—all admitting the fierce and cutting winds of spring. Rotten roofs of thatch letting in the rain, and floors of clay worn in holes, catching the water, and forming little pools, wetting the poor sick child's feet as he scrambled or was lifted in or out of bed. Miserable fires, and no proper

bed-clothes, want of the means of either cleanliness or nourishment, joined to a total ignorance of nursing or cooking, rendered treatment, to a great extent, formal. Later on when, local attention being aroused, a fund was formed, I was enabled to distribute a liberal supply of coal to all those houses of the poor where the children were ill with the disease, and then the fatality notably decreased. We had, on the whole, a mortality slightly below the average (one in fifteen) of modern writers, there being as closely as could be ascertained 220 cases, with 11 deaths, or an average mortality of 5 per cent., or one in twenty.

In conclusion, with reference to a sometime disputed question, as to whether an individual can have the measles twice, I extract the following entries from my note-book, both relating to the same child:—

Measles.—F. H., aged six months. January 8th.—Cough and vomiting. January 12th.—Rash thick on body and legs, light on face. *Prominent symptoms.*—Slight dyspepsia, with perspiration just before eruption; declining on January 15th. *Complications and sequelæ.*—Very severe attack of catarrhal fever. *Remarks.*—A second patchy rash appeared after some days, followed by a crop of pimples, principally on right side of neck and on the legs.

Measles.—F. H., aged eight months. March 4th.—Coryza and cough. March 8th.—Thick close rash. *Prominent symptoms.*—Cough and fever; declining on March 12th. *Complications and sequelæ.*—Very ill, with cough and fever for ten days, until 22nd. *Remarks.*—Though much emaciated, he gradually recovered; but a day or two after the fever left him a thick papillary rash appeared on his legs and body, and slowly declined, with great itching.

During the interval between his two attacks, his cradle was constantly at the bed-side, where all his brothers and sisters, seven in number, gradually went through the disease. He was often nursed by them when their own eruption was out, or coming out, as it was imagined that he was protected by his first attacks from any chance of a second infection. It would appear that in this way the delicate and irritable skin imbibed a second dose of the poison from direct contact, large enough and strong enough to overcome the protective agency of the primary inoculation, the room atmosphere being at the same time loaded with infectious particles.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Handbook of Practice of Medicine. By M. CHARTERIS, M.D.; Professor of Practice of Medicine, Anderson's College, Glasgow; and Physician to, and Lecturer in Clinical Medicine, Glasgow Royal Infirmary. London: J. and A. Churchill. 1877. Pp. 283.

THIS handy little volume is one of the Messrs. Churchill's Student's Guide Series. The author has condensed in a wonderfully small compass a large amount of information with which students may be safely trusted to supplement their bed-side instruction. Dr. Charteris, a medical teacher himself, has succeeded in writing as one speaking to his pupils. Everything is put in the clearest and least ambiguous language, and the interrogatory form occasionally adopted serves to impress important points.

Commencing with some useful "Bed-side Hints," the author proceeds to describe the fevers—the etiology, symptoms, diagnosis, prognosis, pathology, and treatment being discussed in order. The treatment of hyperpyrexia in fever by cold immersion is referred to with approval, but is not alluded to in connexion with the high temperature mentioned as sometimes proving fatal in acute rheumatism. In the latter disease the good advice is given that the patient should always lie between blankets, and salicin and salicylic acid are spoken of as being most beneficial. The relief often obtained by placing the affected joints on light splints, and by injecting morphia hypodermically in their vicinity, might perhaps have been also mentioned. All through the work, however, the therapeutics seem to be both commendable and modern, and a well-stocked appendix of useful formulæ is constantly referred to in the text. The treatment of aneurism by Tufnell's method, by the administration of the iodide of potassium, and by galvanopuncture, is described.

In the section on Gout we learn incidentally that this disease is never seen in Scottish infirmaries; and in that which treats of Diabetes, that the parched state of the mouth may be relieved, and

perspiration induced, by wearing a respirator night and day, over which may be placed a knitted woollen cloth.

The introduction of diagrammatical engravings render the author's excellent description of the cardiac and pulmonary auscultatory phenomena very clear. Other engravings—*e.g.*, of the ordinary urinary sediments, and of the *tænia echinococcus*, &c.—make the book one of the best of its kind we are acquainted with. Many diseases not referred to in ordinary text-books are here briefly and well described.

An Essay on the Action of Free Phosphorus and the Hypophosphites.

By S. R. PERCY, M.D. New York. 1877.

THE burden of this pamphlet appears to be a tirade against Dr. Ashburton Thompson's assertions, and especially his chemical statements. The author's own opinion respecting the propriety of giving free phosphorus may be gathered from the following sentence (p. 23)—“Upon consulting the ‘Table of Mortality’ of the Health Department of the City of New York, I find that the number of deaths from diseases of the kidneys, for the year 1874, was 975; the number of deaths for 1875 was 1,108—138 more than the previous year. Is this increase due to the use of phosphorus pills?” (!) And again (p. 22)—“Let us hope that this latest fashionable folly will not result in multiplying our cases of Bright's disease.”

A Practical Treatise on Aural Surgery. By H. MACNAUGHTON JONES, M.D. London: J. & A. Churchill, 1878. Crown 8vo. Pp. 172.

THIS is a wonderful little book. In 172 crown-8vo pages, not very closely printed, the author gives us a good idea of the present state of Aural Surgery. Most of the burning questions of the day in the speciality are touched upon, and those which are likely to be of importance to the general practitioner are considered at greater length. Treatment occupies a large share of attention, and Dr. Jones' experience renders this a very strong point in the book. Having practically tested all the modern methods, he has been able to form an opinion concerning them, and can speak with authority as to their usefulness or otherwise. If every practitioner would

read and duly apply the short paragraph on Foreign Bodies in the External Meatus alone, much good might result. We fancy the arrangement of the book might have been better. Possibly, however, this defect depends upon the great effort at conciseness which the author has made. The illustrations are abundant, and type and paper are good.

Lessons in Laryngoscopy. By PROSSER JAMES, M.D., F.R.C.P., &c. Second Edition. Pp. 176. London: Baillière, Tindall, and Cox. 1878.

THE second edition of this handbook on Laryngoscopy differs in no respect from the first edition, except that the plates collected at the end of the book have been coloured. This adds to the value of the book, for the plates are well executed and characteristic; we regret only that they are so few. The first two chapters are devoted to the mechanical appliances in use in laryngoscopy. The next three chapters treat of the manner in which the image is produced. These chapters are clearly written, and will prove of use to beginners, for whom the book is specially designed. Chap. VI. is explanatory of the parts seen in the laryngeal image, whilst Chap. VII. mentions the different ways in which these parts may be demonstrated. Chap. VIII. gives a short account of Rhinoscopy, and though it contains some points of practical interest, yet, from the superficial treatment the subject has received, but little information is to be derived from its perusal. The next two chapters are devoted to the history of laryngoscopy, Chap. IX. consisting chiefly of a reprint of Manuel Garcia's paper, communicated to the Royal Society in 1855, on "Observations on the Human Voice." In Chap. XI. Dr. James enumerates very briefly the changes in colour and in form met with in the larynx, and the diseases of which these changes are indicative. The rest of the book is therapeutical. This latter portion, treating of the various methods of applying curative measures to the larynx, is unconnected with the chapter on the diseased conditions, so that the reader is at a loss to determine in what cases the different therapeutic agents are recommended. Thus he enumerates the solutions which he employs, the powders he uses with the insufflator, the methods of applying solid caustics to the larynx, the uses of atomised fluids, &c.; but the cases in which any one of these is preferable to another, and the reasons which should guide the

operator in his selection, are left unfortunately to other, and larger, text-books to elucidate. This is a point which, we think, Dr. James will do well to revise, should he contemplate a third edition; and should he wish to retain the volume within its present dimensions, we think he would make it more generally useful were he to sacrifice some of the historical claims to priority of invention to the more practical and instructive part of laying before the learner some more specific hints as to the treatment of the diseases with which he may come in contact.

Medicinal Plants. By R. BENTLEY, F.L.S., and H. TRIMEN, M.B.; F.L.S. London: J. & A. Churchill. Parts 22 to 31. 1877-8.

MANY of the plates in these parts represent non-official plants (B.P.), while we also recognise many familiar faces.

The high standard of artistic execution is fully maintained as compared with previous parts; and, indeed, we may add that the style and finish of the drawings appear to improve as the work advances.

The promoters are to be congratulated upon the continued success of their important undertaking, and upon the regularity with which the numbers are issued, and we anticipate a cordial appreciation of their labours from all who are interested in the scientific study of the *materia medica*.

Dublin Hospital Sunday Fund. Report of the Council for the year 1877. Dublin: Browne and Nolan. 1878. 8vo. Pp. 43.

LAST year we congratulated the Council of this fund on the success of their charitable labours. We are happy to be able to reiterate our congratulations this year, for the collection, which was made on Sunday, November 11, 1877, amounted to £4,143 19s. 6d.—a very satisfactory increase of £270 10s. 3d. over the collection made in 1876. The Council divided a sum of £3,900 amongst the participating hospitals—15 in number, the Dublin Orthopædic Hospital being included for the first time. The collection in 1877 was made in 234 places of worship, being an increase of 21 compared with the previous year, and of 39 compared with 1875.

The Council observed with regret a falling off in the amount of subscriptions to the hospitals as compared with 1876. But they

believe that this falling off is due to the general depression of business, which has produced a similar result in the case of most of the public charities of the United Kingdom.

The tables included in the Report are as instructive as usual, and will be indispensable to anyone who wishes to make himself acquainted with the financial and general management of the Dublin Hospitals.

Journal of the Scottish Meteorological Society. New Series, Nos. LI.—LIV. Edinburgh and London: Wm. Blackwood & Sons. 1878.

THIS quarterly publication maintains its high standard of excellence. The present numbers contain an interesting and novel communication by Professor C. Piazzì Smith on Rain-band Spectroscopy, from which it appears that the spectroscope is likely to prove one of the most useful instruments for purposes of weather-forecasts. There is no specially medical topic treated of in these numbers of the journal.

Tables of Materia Medica. BY T. LAUDER BRUNTON, M.D., F.R.S. London: Smith, Elder, & Co. 1877. Pp. 204.

THIS work appears as the forerunner of a text-book which the author hopes to issue shortly, and which will treat more fully of the physiological action and uses of drugs. We greatly doubt if the present volume will fulfil the praiseworthy objects aimed at by its distinguished author—viz., to help the student to recall what he has learned from larger text-books, and to aid him in arranging in a convenient order the knowledge he has thus acquired.

Following Drs. Harvey and Davidson, an attempt is made to indicate the relative value of the drugs by the use of different sorts of type, but the plan appears to us to be carried out in an unmeaning and arbitrary manner, and no definite clue to it is furnished. If the work, as the writer confesses, was one of tedious and weary compilation, it is also a wearisome book to use, and the scheme of study proposed in the introduction is, we fear, quite unlikely to be adopted by the most conscientious student. In the author's next literary effort we shall confidently look for a result more worthy of his acknowledged powers, and of his high reputation.

PART III.

HALF-YEARLY REPORTS.

REPORT ON OPHTHALMOLOGY.

By C. E. FITZGERALD, M.D., Ch.M. (Dub); Surgeon Oculist-in-Ordinary to the Queen in Ireland; Surgeon to the National Eye and Ear Infirmary; Ophthalmic Surgeon to the Richmond Hospital; Lecturer on Ophthalmic Surgery, Carmichael School of Medicine; Extern University Examiner in Ophthalmic Surgery.

I.—GLAUCOMA AND INCREASED TENSION OF THE GLOBE.

THE past year will mark an epoch in the history of the progress of ophthalmology, for in it we see the first signs of dawn lighting up the horizon of a hitherto impenetrably dark region. We allude to the remarkable communications which have appeared upon the subject of glaucoma. The exact nature and etiology of this disease has up to this proved one of the most difficult problems in the whole range of ophthalmology. We feel confident that its solution will be reached at no very distant date, but as yet we only discern the preliminary steps towards it. Consequently it appears advisable to postpone any lengthened account of the various investigations and experiments which have been undertaken with reference to this intricate subject, and in the present instance merely to indicate very briefly the general tendency of the conclusions hitherto arrived at. On one point there seems to be almost complete accord, as evidenced by the papers of Kniess^a and Weber^b, as well as by the opinions expressed at the Heidelberg meeting—namely, that the chief factor in the production of the disease is an interference with the “drainage system,” if we may so term it, of the eye. This opinion is principally grounded upon the results of pathological investigations. Kniess insists that these investigations prove the important rôle which the obliteration of Fontana’s canal plays in

^a Archiv. f. Ophth. Bd. XXII. Abth. III. P. 163. Bd. XXIII. Abth. II. P. 62.

^b Archiv. f. Ophth. Bd. XXIII. Abth. I. P. 1.

the production of the glaucomatous state; and Weber concludes that in all forms of glaucoma—the inflammatory and non-inflammatory as well as the primary and secondary—the drainage channels become narrowed and finally closed.

At the meeting of the Ophthalmological Society at Heidelberg last year, Dr. Stilling^a stated the results of some experiments he had made on ligaturing the optic nerve. At first this proceeding was invariably followed by neuro-paralytic symptoms. After five or ten days had elapsed the tension enormously increased; there was complete anæsthesia of the cornea, the eye was stony hard, and the vitreous, especially its posterior portion, became quite fluid. He proposed dividing glaucoma into two groups:—1. Glaucoma anticum—due to some obstruction in the anterior drainage system as well as Fontana's canal. 2. Glaucoma posticum—depending on an obstruction in the vascular sheath (pial Scheide) of the optic nerve.

In the "Royal London Ophthalmic Hospital Reports," Vol. IX., Part II., 1877, Mr. Brailey, curator of the museum, contributes an interesting and suggestive paper on the Pathology of Increased Tension of the Globe. His observations are based upon the microscopic examination of 53 cases, in which increased intra-ocular tension had been at some time a symptom. He divides his cases into two great groups:—

"A. Those in which an affection of the iris has been the primary cause of the tension, the disease either remaining confined to this—as in two cases of buphthalmos—or extending backwards so as to affect the ciliary-choroidal tract secondarily."

"B. Those in which the part of the tract posterior to the iris is affected, without any obvious iritic change preceding it."

The first group numbered 28 and the second 25 cases. The latter included "all cases known usually as glaucoma."

The seat of the most marked and universal changes occurring in glaucoma, was found by the author to be in the ciliary body. Of the cases included under group B, it was extensively affected in all but one, and in it there was some change. In group A it was markedly implicated in all but three.

"The remaining 49 cases of increased tension present marked ciliary changes, and the one which is present in all, and more distinctly seen in each than any other pathological condition, is atrophy of the ciliary muscle."

The circular fibres were specially affected, and to such an extent

^a Bericht der Ophthalmologischen Gesellschaft. Heidelberg. 1877. P. 16.

that in many cases the affected part presented no traces of muscular fibres at all. At the same time the radial fibres did not escape, their bulk being diminished, though to a less extent than the others.

The author alludes to the peripheral adhesion of the iris, a condition to which considerable attention has been directed in the papers by Kniess and Weber, and also in the discussion on Pagenstecher's communication at the Heidelberg meeting. He does not agree with Kniess that it is the cause of the increase of tension, though it may undoubtedly have a "great effect in keeping up an already established increased pressure." He also considers that Kniess attaches too much importance to the closure of Schlemm's canal. While admitting that in most of his specimens of increased tension Schlemm's canal was commonly closed, yet it was by no means invariably the case, and it was a much less constant condition than either atrophy of the ciliary muscle or the atrophy or adhesion of the iris. He is disposed "to attribute its closure to its compression, either through the dragging forward of the iris in a prolapse, or its bulging forwards from pressure behind, as in the cases of group B."

One very suggestive passage towards the close of the paper is worthy of note:—"In studying longitudinal sections of the atrophied ciliary muscles of glaucomatous eyes it is very common to find sections of dilated empty venous channels. These are of more frequent occurrence than the changes in Schlemm's canal, but are not so constant as the ciliary and iritic changes above described. I am unable to say precisely what may be the pathological causes or results of this condition, but the atrophy of the muscle may be connected with yielding of the muscular tissue surrounding its veins. The increased intraocular pressure should have a tendency to close these veins. We may connect them with the dilated ciliary veins so often seen externally during life in such cases."

"It is extremely likely that the ciliary muscle has functions besides that of accommodation, and that it may be concerned in regulating the blood supply. This might explain the effect of an atrophied ciliary muscle in passively keeping up an increased tension, notwithstanding that the periphery of the iris has never been united to the cornea."

II.—PROGRESSIVE MYOPIA AND POSTERIOR STAPHYLOMA.

Dr. Edward G. Loring, of New York, has published a most interesting paper on this subject, and which is reprinted from the

Transactions of the International Medical Congress, Philadelphia, September, 1876. The question he proposes is—are progressive myopia and conus (posterior staphyloma) due to hereditary predisposition, or can they be induced by defect of refraction acting through the influence of the ciliary muscle?

After careful consideration he has come to the conclusion:—

“(1) That hereditary predisposition, though undoubtedly a potent cause, is not only not the sole cause, but not even the predominating cause, of progressive myopia; and (2) that the action of the ciliary muscle, taken by itself, exerts but little influence on the production of myopia, and still less in the formation of the cone.”

The author points out how the belief that like begets like dates from the most remote periods, but that nevertheless from time to time grave objections and very weighty arguments have been advanced against it, and that these would specially “go to show that the effect of direct transmission could not be so readily detected, nor so clearly demonstrated, in regard to the special senses as to the organism as a whole.”

He cites a number of authors to prove the prevalence of the opinion that myopia is due to hereditary influence, from Lucas (1647) to Donders (1864). The latter affirms that “the predisposition is almost always congenital, and in that case it is, moreover, nearly always hereditary. Beer, Jüngken, Bohn, Von Hasner, and many others, have referred to its hereditary nature, and I believe even that from time immemorial the conviction thereof has been general among the people.”

It would be supposed, as Loring justly argues, that opinions such as these would be based on exact and extensive statistical information, but such is not forthcoming. The author then draws attention to the fact that there is a much greater proneness of the mind being struck with resemblance in such matters (as, for instance, the frequency with which myopic parents bring to us their myopic children) rather than dissimilarity—as, for example, the frequency with which “children with myopia are brought by parents who, on actual examination, are shown to have normal eyes, and whose ancestors on both sides, as far as known, were never near-sighted.” Among 715 well-educated and intelligent persons whom the author examined for this special purpose, he found “that the percentage of emmetropic children from myopic parents was as high as that of myopic children from myopic parents.”

The investigations of Cohn appear quite to corroborate this, and though the percentage found by Erismann in his statistics is much higher than either Loring's or Cohn's; yet taking it "as a basis, we find even then that two-thirds, or seventy per cent., of the myopic children had parents who were not myopic."

In the absence of exact statistical knowledge, the author nevertheless considers there are "several factors of less importance, it is true, but still of sufficient force to aid us materially in the solution of the problem, and amongst these the most important are necessarily those of an anatomical nature." He then proceeds to deal with the question of the condition known as *conus* or posterior staphyloma, and he concludes that the rather sweeping assertion that posterior staphyloma and myopia are nearly synonymous terms (Donders) is somewhat exaggerated. But of 500 myopic eyes he found only 20·5 per cent. affected with the crescent, and the statistics of Cohn and Max Conrad show an almost identical percentage. Though forced, then, to the conclusion that there is an anatomical variation predominating in myopic over other eyes, it would nevertheless appear to be considerably overrated; nor should too much weight be put upon this fact as an argument as to the congenital and hereditary character of the cone, since, in the first place, an anomaly which makes its appearance in only about one-fourth of the cases, and under the most favourable auspices for its development—that is, in school children between the ages of six and twenty-one years—cannot be said to be strongly congenital.

The second anatomical variation which the author draws attention to is "the peculiar conformation of the ciliary muscle, which is supposed to be congenital." The researches of Iwanoff show that the muscle in the myopic eye extends further back along the sclera than in the normal eye, and is, moreover, broader; and as regards its component parts, that the circular fibres are very much reduced or entirely wanting, whilst the longitudinal have become hypertrophied. The author then criticises the theory which—assuming that the circular fibres have undergone atrophy from disease, as there is no demand for active accommodative efforts in the myopic eye, implies that the action of the muscle has changed, and from being a compound muscle, has become what Iwanoff terms a pure "tensor choroideæ." "If we should accept this theory," says Dr. Loring, "we would have the apparent *reductio ad absurdum*, as the author himself says, in the development of a force which he supposes to produce active accommodation in an

eye which does not require or use it; for traction on the choroid, and consequent relaxation of the zonula, is supposed by Helmholtz and other physiologists to be the important element in the production of accommodation." Dr. Loring considers that a very important factor, and one which might probably exert a considerable influence in altering the form and shape of the muscle, has been neglected—namely, the effect of distension of the investing membranes of the eye, which moreover in some cases is very great—as, for example, in progressive myopia developed very rapidly to a very high degree. Thus, while not denying the hereditary tendency towards a too yielding sclera, he is of opinion that the cone may be due in many cases to the purely mechanical effect of distension. That the emmetropic or normal eye may pass into a myopic one, appears to be pretty generally admitted, but that a hypermetropic eye may become a normal or myopic eye, is still a matter of doubt with many of the best authorities. In the absence of sufficient direct proof of the passage of emmetropia into myopia, and the almost total want of the same as regards hypermetropia, we are forced to take collateral evidence such as is "furnished by statistics taken from large masses of individuals, which show the proportionate rate in which the different classes of refraction occur at different times of life, and with different degrees and kind of application of the eyes."

The statistics he selects are Erismann's, compiled at St. Petersburg from the examination of the eyes of 4,358 scholars; Conrad's at Königsberg from 3,036 eyes; and Dr. Derby's and his own from 2,265 eyes in New York. An examination of these statistics shows that there is less myopia in America amongst school-children than in either Russia or Germany, and that the Germans, as a nation, exhibit a strong tendency to myopia.

Dr. Loring concludes that from these and such like statistics, made in different countries and by different observers, he cannot, "while admitting the hereditary tendency, see how we can exclude the fact that there are many eyes which, under prolonged tension of the accommodation, pass from hypermetropia to emmetropia, and thence to myopia with all its attending signs and symptoms, and this, too, in spite of hereditary influence to the contrary."

The second part of the question—namely, whether posterior staphyloma and progressive myopia can be produced through the influence of the ciliary muscle, is next discussed.

In two ways the ciliary muscle might be supposed to render an

eye, not previously near-sighted, myopic. "The first would be by increasing the curvature of the lens by active, that is, positive, contraction; and the second by producing an elongation of the antero-posterior axis."

The former, though held by some—as, for instance, Jaeger and Stellwag von Carion—to be a potent cause in the production of myopia, is not now regarded by the majority in that light, and, indeed, the proofs, both experimental and clinical, would actually appear to show that it has no existence.

The arguments against the second action of the ciliary muscle, which supposes increased curvature of the lens, "followed by irritation of, and traction on, the deeper-seated membranes, which lead to true myopia—that is, to an elongation of the axis," may be summed up as follows:—

1. The signs of irritation should be most frequently met with where there is the greatest strain on the muscle—that is to say, in hypermetropic eyes. This, however, is not the case. Here, also, the traction ought to produce the cone, but the latter is found less frequently in hypermetropia.

2. The cone should be found most frequently at the inside of the nerve, "since this would be the place of all others in the circumference of the nerve where the traction would be soonest and most powerfully felt, for the simple reason that it is the shortest line between the two points of attachment." In the majority of cases, however, the cone is to the outside.

3. Negative proof against the theory is supplied by the fact that "the greater the myopia the less the positive contraction of the ciliary muscle."

At the close of his paper, Dr. Loring condenses his conclusions thus:—

- "1. From the fact that there is so large a percentage of children who are myopic, but whose parents are not near-sighted, while myopia increases directly with the amount of increased tension of the eyes, and from the fact that an interchange of refraction may occur, whereby an eye which is not congenitally myopic may become so in spite of hereditary tendency, it would seem to follow that hereditary predisposition, though undoubtedly a potent cause, is not only not the sole cause, but is not even the predominating cause of myopia.

- "2. In regard to the second question as to whether the ciliary muscle acting through faulty refraction can produce myopia and

the cone, I conclude that the action of the ciliary muscle, taken by itself, exerts but little influence on the production of myopia, and still less on the formation of the cone."

III.—THE INFLUENCE OF MODERN EDUCATION UPON THE FORM OF THE EYE.

In close connexion with the previous subject, we find the same author proposing and discussing, in a paper read before the American Society of Social Science and the County Medical Society of New York, a question of the greatest interest, both from a social and a scientific point of view—viz., Is the Human Eye Changing its Form under the Influence of Modern Education?

Though believing firmly in the hereditary tendency of myopia, the author nevertheless thinks that too great an influence has been attached to the effects of heredity upon the organs of vision. He considers that this is chiefly due to the want of attention to the law of heredity, which is known as the law of reversion, that is to say, "the return of an organ, which has been modified, to what it was in a preceding generation before such modification took place." This law is held especially to apply to the higher grade of animals.

The normal human eye, considered merely as regards its optical condition, that is to say, its power of bringing parallel rays of light to a focus upon the retina without any effort, may be looked upon as having reached perfection. Supposing then any accidental modification to have taken place in this condition, the author conceives that there is a strong tendency to revert to this standard. What exactly governs this return of deviation from the type to a normal state is almost unknown, but we cannot help believing that it is largely due to, and is in proportion to the perfection of the organ. So, for instance, "the comparatively few and sporadic cases of near-sightedness, brought about by fortuitous circumstances, would rapidly become extinct, through hereditary influence, by intermixture with a greater number of normal eyes." To put it somewhat differently, he believes "that the tendency to revert to a far-seeing standard, perfected in the remote past, and maintained probably for cycles of time, would outweigh the tendency to propagate a myopic variation, *unless a marked change in 'the conditions of existence'—a potent factor in the production of variations—had also taken place—a change which should be brought to bear not only on a few individuals but on the great masses of a community, and not only on one sex, but on both sexes alike.*" That such a

change of conditions exists he considers we have no further to look than "to the fact of modern devotion to literary pursuits, and especially to that phase of it known as 'compulsory education,' a factor of modern origin, of great force, and of an exceedingly rapid growth."

The question of the effect of prolonged tension and over-use of the eyes is then considered, and the author brings forward for reconsideration the same statistics as in the preceding paper. These would seem to prove conclusively one fact at all events—namely, that there is amongst the German scholars generally a far larger proportion of myopia than amongst the Russian or American. Further, as it must be admitted that the German is certainly one of the most studious nations in the world, "it would seem to follow directly that the near-sightedness was due entirely to the excessive use of the eyes on books." The author, however, by no means readily agrees in this opinion, for he believes "that there are other factors in the production of myopia prevalent to a degree in Germany which do not exist to the same extent in any so-called cultivated race, such as peculiarities of food, indifference to ventilation, disregard of other hygienic requirements, want of out-of-door exercise, and a peculiar tendency towards a sedentary life, all of which are provocative of a certain laxity of tissue, and want of resistance in the investing membranes, which finds its expression in the eye in a distension which is in fact myopia."

The statement of Donders that near-sightedness is greater amongst studious nations would appear, both from statistics and facts, to be virtually true. Logically then it ought to follow "that where the near-sightedness in a nation was less, the cultivation and literary achievement would also be less." This, however, the author questions, and he warmly challenges the laudatory encomiums of Mr. Matthew Arnold in favour of the German education system as compared with the English school teaching. He triumphantly points to the long list of illustrious writers and scientists from Shakespear, Bacon, and Newton down to Huxley, Darwin, and Thompson; and asks can all the other nations of the earth combined, the American included, offer such an array of names in any department of literature.

Before determining the cause of the increase of near-sightedness and the best method of preventing it, the author sums up the leading facts which he has brought forward in his paper, and the conclusions he would draw from them. And first of all, he thinks,

we are safe in assuming, from the authority of so many who are competent to judge, that near-sightedness is hereditary, though the degree to which it is so has not been accurately determined; and secondly, that myopia is produced even where there is no hereditary tendency, by excessive application of the eyes in study, and that, other things being equal, the frequency is proportionate to the amount of application."

But admitting all this, he doubts whether more than a conditional argument can be given to the following passage from Ribot—namely, that "since constant study creates myopia, and heredity most frequently perpetuates it, the number of short-sighted persons must *necessarily* increase in a nation devoted to intellectual pursuits." If by a nation devoted to intellectual pursuits is meant that a compulsory education is to be carried out to the fullest extent, and that Germany be taken as the type whose example every nation desirous of intellectual progress should be compelled to follow, then he is "of the opinion that not only the educated classes, as the term is commonly understood at present, but that the world at large will, in time, become near-sighted." He conceives there are special grounds for apprehension with regard to his own nation, because he looks upon the question as really one of *conditions*. Like the English, however, the Americans have a passionate fondness for out-door games and athletic exercises, and whether this, "together with a better understanding and enforcement of hygienic laws, will suffice to prevent an increase in the amount of near-sightedness, can only be told by future examinations made at long intervals."

Of one fact Dr. Loring appears perfectly satisfied, and that is, that the amount of myopia may be lessened by changing the period of life at which the greatest amount of application of the eyes is made. It seems to be established beyond all doubt that myopia "is essentially a disease of childhood, or, at the latest, of adolescent life." The great period for its development or beginning is from the tenth to the fifteenth year, when the rest of the body is also developing. The eye, like other parts of the body, is then more elastic, and liable to yield to any strain; consequently, it would seem that the simplest, and indeed "the only method of preventing an increase in near-sightedness, if the present high standard of instruction is to be preserved in force, is to lessen the amount of work done by school-children during the period of life from eight to sixteen years, and to restore the equilibrium, if

necessary, by increasing the amount of study after that time, or better still, by increasing the period of time devoted to study."

Before concluding he alludes to one matter which, at first sight, might appear contradictory of the fact that myopia is produced by long-continued work on small objects—namely, the comparative rarity of myopia amongst those pursuing mechanical callings—such, for instance, as watchmakers, when compared with those devoted to intellectual pursuits. He believes, however, that this is really due to the fact that, in the case of the former, "the application of the eyes occurs at a different time of life, and under entirely different surroundings or conditions of existence."

In conclusion he makes the following pithy remark:—"Two questions have presented themselves to my mind while making my investigations on near-sightedness in the public schools. One is, whether the word instruction is always synonymous with education; and the second is, whether, while we are reducing the number of the absolutely blind in our asylums by improved methods of operation and treatment, we are not, by over-use of the eyes at school, laying up a future evil which, though milder in form, will, from its very frequency, entail a greater and more lasting detriment upon the race."

CAUSE OF THE SO-CALLED URÆMIC CONVULSIONS.

MM. FELTZ and RITTER presented a note to the Académie des Sciences on the 15th of April last, in which they adduce experiments showing that pure urea never causes convulsions. They conclude as follows:—Pure urea, artificial or normal, injected in very full doses into the venous system, never causes convulsive symptoms; it is rapidly eliminated by the secretions. Normal blood does not contain any ferment which could change urea into ammoniacal salts; rapidity of the elimination cannot be cited as a cause of this non-conversion, since by suppressing the renal secretion the elimination of urea may be retarded without hastening the supervention of the eclampsia. The ureas which, in large doses, cause convulsions are always impure ureas, containing ammoniacal salts, the presence of which can always be detected by Nessler's test.—*Gaz. Méd. de Paris*, April 27.

PART IV.
MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE
COLLEGE OF PHYSICIANS.

SAMUEL GORDON, M.D., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, May 1st, 1878.

DR. AQUILLA SMITH in the Chair.

DR. GRIMSHAW read a paper "On the Prevalence of Small-pox." [It will be found at p. 490.] He said the questions on which he wished to elicit the opinions of members of the Society were—How the epidemic arose? how it had been promoted? how it might be ameliorated? and how future epidemics of the same sort might be prevented? In reply to a question, he stated that 20 per cent. of the vaccinations in the Dublin district were performed in the Cow Pock Institution.

The CHAIRMAN said the Table which had been submitted to them confirmed what the experiences of all had proved as to the great efficacy of vaccination, not only in rendering small-pox much rarer than it had been, but also in mitigating it in a very remarkable way.

DR. HAYDEN said the paper tended rather to confirm previous views which had been entertained on the subject than to give much new light. The first point that had struck him was, what it proved as to the great value of vaccination rather in lessening mortality than in preventing attacks of the disease. There could not be a doubt that where persons had been previously vaccinated the mortality was less than it would otherwise have been. On the other hand every year's experience tended to show that the value of vaccination as a preventive measure was less than had been supposed. There was, therefore, no reason for discouraging the practice. In his limited experience of the disease, extending over only 57 cases—being the total number that had been admitted into the hospital with which he was connected since the 9th of last February—he had been astonished at the number of adults that

exhibited no marks of vaccination. It was therefore very necessary to encourage the adult population to avail themselves of the great facilities that now existed for re-vaccination. A few days ago he visited the Vaccination Institution in Sackville-street, and was greatly edified at the number of adults he saw there waiting to be vaccinated. As to the origin of the epidemic, he thought there could be but the one opinion. It had been imported. Then it was clear that it had a favourable soil to nestle in, and had become rapidly developed, owing to the unprepared state of the city as regarded vaccination and isolation. How it had been promoted was clear. People were not aware of the danger of intercourse with persons suffering from the disease. In two or three cases he had seen the most extraordinary reluctance on the part of patients to be sent to hospital, their main object being apparently to elude the police, who were on the watch to oblige them to go to hospital. There was also great danger from convalescent patients, as there was a period when the patient was quite well and yet unsafe to mix with the community. Poor people returned home in the stage of desquamation, and with their clothes infected, and there could not be a doubt that the disease had been extensively propagated by the contact of others with them. He was glad to observe that the Poor Law Boards had established convalescent institutions for the purpose of isolating those poor people for a considerable time. He trusted that those institutions contained adequate arrangements for giving baths and for the disinfection of clothing. The question as to how the epidemic could be prevented was a very large one; and on that point Dr. Grimshaw had not given them much information, except impliedly. No doubt they should be on the watch to prevent the importation of an epidemic; but as that was almost impossible, in the present active condition of commercial intercourse, they should fall back on preventive measures of another kind. In the first place people should be fortified by both vaccination and re-vaccination. According to his experience re-vaccination was of great value, but he did not think it conferred complete immunity beyond a period of from six to eight years. Recently he had attended, in the city, a lady, aged about twenty-two, who died of small-pox. She had been thoroughly vaccinated when a child. She was re-vaccinated during the last epidemic of small-pox (1871), and the vaccination failed, which led her friends to conclude that she was protected. She nevertheless contracted the disease in the most virulent form, and died on the ninth day. He had seen other cases which pointed to the conclusion that those who were especially exposed to the contagion of small-pox should be, if possible, re-vaccinated every six or seven years.

DR. LALOR said it seemed to be the general opinion that the best means of preventing the extension of small-pox was to prevent susceptibility by vaccination and re-vaccination, and also to provide convalescent

hospitals, and cut off the sources of the disease. In the returns submitted he did not think a distinction had been taken between the number of cases admitted into the hospitals and the number of persons in hospital who contracted small-pox from other patients. He believed that a large number of convalescent hospitals would be required to provide for all the convalescent cases until they ceased to be sources of contagion. His impression was that the period during which one person might communicate small-pox to another extended over months, and that the liability to contagion continued until the skin was cleared of all desquamation. He need not state how long that was in the case of a person who had suffered from bad confluent small-pox. Dr. Hayden had remarked that in the convalescent hospitals it was most important that clothes should be disinfected. If that were merely done at the time of their admission to the convalescent hospitals, and they remained there for a month or six weeks, and the process were not repeated before they left it, there would be danger of contagion. The clothes should be again disinfected at the final period of discharge. Another question was as to the possibility of disinfecting the person of the patient—of disinfecting the scales upon his person. He believed there was no known means of doing so, but means might be devised.

DR. MACSWINEY observed that formerly the practice of inoculation was favoured by the State—now it was illegal. Nevertheless he believed that it was still secretly practised; and it had been recently stated that three cases of small-pox occurred in the country which were attributable to it. Notwithstanding the apparent clearness of the proof that the present epidemic had been introduced from England, it was possible that it might have arisen in some quarters from inoculation. It was a popular medical opinion that re-vaccination was a potent means of conferring comparative immunity from attacks of the disease; but sometimes the re-vaccination did not take. Allowance, however, should be made for the possibility that the re-vaccination might not have been properly performed. A remarkable instance of that had come under his notice within the last few weeks. Therefore, it was always well to repeat the operation in the event of its failing in the first instance.

DR. ASHE said that four years ago there was a very severe epidemic of small-pox in Londonderry Asylum, and a large number of cases occurred. He vaccinated the people as rapidly as he could. There was a night-nurse in the asylum who stated to him that she had small-pox when she was very young, and showed him one or two pits. He vaccinated her, but the lymph failed to operate. He repeated the operation twice afterwards with the same result. She then attended the patients as a nurse; and when the house was completely clear of the disease, she took it, and her case was one of the worst of malignant small-pox that ever he saw, being accompanied with hæmorrhagic complications of the

most frightful kind from every mucous surface. He succeeded in arresting the hæmorrhage after two or three days, but lung complications set in, and the woman died.

DR. HENRY KENNEDY said he wished he could put implicit confidence in vaccination as regarded the keeping away of small-pox. Dr. Grimshaw seemed to argue that precisely as vaccination was carried out, in the same proportion would small-pox decline. He (Dr. Kennedy) could not accept that view. Epidemics of small-pox had come and gone, just as at present, long before vaccination was adopted. Besides, in the later epidemics, those vaccinated, even in the best positions, were by no means free from attacks of small-pox, and a mortality of even 9 per cent. had occurred amongst them. In his opinion the effect of vaccination was the modification of the disease, and that it did not seem possible to contravene. He had met with pustules in the arch of the aorta on its inner surface, and also in the mucous membrane of the bladder. As to the age at which small-pox might occur, he had at present a lady under his care who was sixty-five years of age, and who had passed through an attack which for several days caused her life to be in danger.

DR. CHURCHILL did not think vaccination could prevent purpuric small-pox. But a mere scrape on a person's arm was not a sign that vaccination had been properly performed—a proper cicatrix should be apparent. They never could get the same vesicle on a person who had been re-vaccinated that they did in the case of a child. He thought that re-vaccination was an exceedingly good operation to perform, partly because it might keep off small-pox, and partly because it showed that the patient was already protected. In nine cases out of ten the failure of vaccination was the fault of the patients, because they would persist in drying their arms at the fire, or wash off the spots of blood, although told not to do so. Medical men sometimes made too large a cut, and let out too much blood. The smallest speck was enough; if they did more, they did mischief.

DR. FINNY said Dr. Grimshaw's Tables did not distinguish between those who had been re-vaccinated since 1874 and those who had been primarily vaccinated since then. He believed that re-vaccination was of the utmost importance in preventing the spread of the disease. In 1869 he was exposed to small-pox for three weeks while attending a member of his family, and another medical man who was in attendance asked him if he had been re-vaccinated, and he said he had not. That gentleman vaccinated him twice, and it failed. He re-vaccinated himself in 1872, and it failed; he repeated the operation in 1875, and it took. He believed that on the last occasion, although the vesicle was small, he had worn out the protection during the preceding five years; and he thought it should not go abroad that the Society were of opinion that they should wait six years for re-vaccination. It was not possible, how-

ever, to lay down a fixed rule as to time. In some cases the effect of the vaccination lasted much longer than in others. He thought the idea that vaccination protected from small-pox was very much exploded, although the mortality in vaccinated cases was very much less than that in cases in which vaccination had not been performed. With regard to the re-vaccination of persons while exposed to the contagion of small-pox, he had a good deal to say to students who were more or less exposed to the disease, and he recommended that they should be re-vaccinated, as he believed the vaccination poison would outrun the small-pox affection, and that if they caught both fevers, the effects would be modified. Isolation of small-pox patients was of utmost importance, and so was the destruction of their clothing. The last should be especially insisted on, as anyone acquainted with the poor knew that they would rather give the clothes of a member of a family, who had died of scarlatina or small-pox, to some other member of the family than destroy them. In former epidemics of small-pox, chicken-pock was very prevalent, and he looked on chicken-pock as a very mild form of small-pox.

The CHAIRMAN said he was sure small-pox pustules would be found in the aorta more frequently than they were, if they were looked for, and also in the mucous membranes. He knew a case of a lady who had natural small-pox five times, and died of the fifth attack. With respect to danger to the eyes, during an experience of thirty years he had seen only one case of the loss of an eye from small-pox. He could not conceive how anyone could question the enormous benefits which had been derived from Jenner's great discovery, when they compared the mortality of the later epidemics of small-pox with those of the epidemics of former years. He entirely concurred in the statement that in a great many instances failure in vaccination arose from want of skill on the part of the vaccinator; and he believed that a great deal of the small-pox they suffered from arose from defective vaccination. He had been informed by midwifery practitioners, including the late Drs. Churchill and Beatty, and Drs. McClinton and Montgomery, that not more than one per cent. of children who had been vaccinated got small-pox. If vaccination were once well performed, he believed there would be no necessity for repeating it. A large mark he regarded as a very questionable sign.

DR. GRIMSHAW (in reply) adhered to his opinion that re-vaccination was a preventive, but did not believe that the most thorough vaccination would protect a person through life from small-pox. As a matter of fact, he had seen typical vaccination marks on persons who had suffered from small-pox, although he admitted that those persons had not suffered severely. Their condition was such, however, that they would have given the small-pox to anybody else. During the last small-pox epidemic two brothers were admitted into Cork-street Hospital, both of whom had been re-vaccinated. They both had purpuric small-pox,

and both died. Exceptional cases of that sort, however, were met with in all diseases and under all circumstances. The occurrence of the purpuric form depended more on the condition of the patient's health, and his previous proclivities, than on anything in the nature of the small-pox poison. This failure of re-vaccination was not any proof of the non-susceptibility of a person to small-pox any more than the failure of the primary vaccination was. Primary vaccinations in numerous cases failed, either on account of the badness of the lymph, or—which was now generally the case—on account of the carelessness of the professional men or of the patients themselves. He was of opinion that small-pox and convalescent patients should be isolated as much as possible. He believed there was no use in disinfecting small-pox clothing; it should be burned. He was glad the authorities of the city had at last adopted a system of destroying the clothing and of providing the patients with new clothing. It was very doubtful whether clothing could be exposed to a high temperature without being injured. A temperature of 200° would not kill lice, and recent experiments had shown that living organisms could even be boiled in alcohol without destroying their vitality. It was very much the custom amongst poor people to pawn the clothes of small-pox patients; and if the patient died and the clothes were not redeemed they were sold to somebody else. He feared that inoculation was still practised in some of the remoter parts of Donegal. Two years ago a smart epidemic of small-pox took place in Galway, the centre of it being in Athenry. He went there and investigated the outbreak, and he had no doubt that it had been caused by inoculation. There were regular inoculations in the west; and the practice was to leave the child on the roadside for them, with a half crown laid beside it, at some distance from the parents' house, and the operation was never seen by anybody, so that a prosecution was impossible. There was no doubt that epidemics of small-pox had occurred from time to time, and that the disease had swept over the country, and attacked all the susceptible persons; and then, until a new generation of susceptible persons had arisen it did not break out again. As to what had been said about aortic pustules, it was now perfectly well known that in the majority of bad cases of small-pox there was ulcerative endocarditis, which, in all probability, was the cause of death. There were no data for ascertaining the number of re-vaccinations throughout Ireland. He had no doubt that re-vaccination put a stop to small-pox. He felt confident that if everybody in Dublin submitted to re-vaccination small-pox would be extinguished in the city in three or four weeks. As to the cases in Cork-street Hospital, the only cases of re-vaccinated persons who had been attacked were those that he had mentioned; and the records of the last few days showed no instance of small-pox occurring in a re-vaccinated person. During the last fortnight he had heard of a lady living on the north side of Dublin,

who had been twice re-vaccinated, being attacked with small-pox, but she had the disease in a very mild form. As to the question of re-vaccinating people in the presence of small-pox, his opinion was that whether small-pox were present or not they should be re-vaccinated. During the last epidemic all the students in Steevens' Hospital were re-vaccinated except five, and those five got the disease, and one of them died. In Cork-street Hospital, on the appearance of the present epidemic, every nurse and servant was re-vaccinated except one, who refused to be re-vaccinated, as she said she was not to be a small-pox nurse, and was about to leave the place. She went to Liverpool, and in three weeks afterwards got small-pox, and had a very severe attack of it indeed. That woman had the vaccine cicatrix. None of the other nurses and servants in the hospital got the disease. He did not believe in chicken-pock. There was comparatively little ophthalmia now compared with what there used to be; and he had never seen serious injury to an eye from small-pox, except in cases of persons who had been previously affected with ophthalmia. Everybody admitted the tendency of small-pox to attack the vascular parts. It attacked the inflamed eyes of strumous persons.

The Society then adjourned for the session.

RECTAL ALIMENTATION AND MEDICATION.

DR. ARMOR, of Brooklyn, New York, writing to the *Medical Record* (Feb. 2, 1878), with reference to a valuable paper recently published by Prof. Austin Flint, sen., makes a practical suggestion of great importance. It is that whatever substance may be selected should be injected into the rectum slowly. The rectum appears to be something like the bladder and other hollow viscera; it does not tolerate sudden distension. Specific directions should always be given to use tepid fluid and inject slowly. With caution on this point it is astonishing how the rectum may be made to tolerate nutritive substances in liquid form. Dr. Armor uses beef-juice, but more frequently milk mingled with beef-blood, expressed out of raw meat, and strained to remove solid particles and fatty matter. When using stimulants by the rectum, he usually adds cream to the milk. Alcoholics in small quantities are readily tolerated in such a vehicle; and even mur. tinct. iron may be administered in liberal doses for a length of time, if the bowel is soothed and shielded by cream. Dr. Armor has seen marked effects from the use of iron in this way alone, and is inclined to think that rectal medication, as well as rectal alimentation, has not received the attention from the profession which it deserves. In a large class of chronic diseases, especially chronic diseases associated with general anæmia, in which the stomach, for some reason, does not tolerate iron (and there are many such cases), it may be used with manifest advantage and for a long time in the mode thus suggested.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

FORTIETH ANNUAL SESSION.

Saturday, 13th April, 1878.

DR. DARBY, President, in the Chair.

Specimen of Tumour of the Labium.

DR. MACAN.—I am indebted to my colleague, Dr. Croly, for being able to show this specimen to the Society. It is a tumour of the labium, and I have been unable to find any case reported which seemed exactly to resemble it. The following are the notes of the case:—Bridget Ledrick, aged thirty years, and married, was, on the 22nd of March, admitted, under Dr. Croly, into the City of Dublin Hospital. She stated that about five years ago she first noticed the tumour, since which date it had been gradually growing larger. When admitted she had a tumour containing fluid hanging from the upper part of her left labium. It was about four inches long, with a pedicle of nearly the same length. It was removed on Tuesday, the 26th. Two ligatures were passed round the pedicle, which was divided between them. There was no hæmorrhage. Carbolic dressing was applied, and she left the hospital on the 8th of April, perfectly cured. The contents of the tumour seemed to be half fluid and half solid, and as it grew from the anterior part of the labium it could not be due to hypertrophy and degeneration of one of Bartholini's glands.

The PRESIDENT.—This is a very interesting kind of tumour, for, as Dr. Macan tells you, it is not of a very common type—at least as far as my experience goes. I saw two cases of it—one in Steevens' Hospital under the care of the late Dr. Hardy, with a pedicle from ten to twelve inches long, and the tumour (about as large as a hen's egg) being down nearly as far as the knee. The tumour was partly fluid, and when felt it gave the sensation to my hand as a bag of worms. It was removed by Dr. Hardy with an *ecraseur*. About six years ago, Dr. Symes, of Kingstown, asked me to assist him in removing a tumour from an unmarried lady. It was of the same character, and was removed with an *ecraseur*. Had I been called upon in Dr. Macan's case I would probably have used the *ecraseur* for its removal. I wish to ask him was any artery felt pulsating in the pedicle?

DR. MACAN—No.

On the Hypodermic Injection of Chloral in Puerperal Eclampsia. By
RICHARD D. PUREFOY, M.B., L.R.C.S.I.

I WISH to lay before the Society brief details of two cases of puerperal convulsions, in the treatment of which chloral hydrate was administered subcutaneously, with results sufficiently good to warrant a further trial of this mode of giving it. The value of this medicine in such cases is now universally admitted; but, so far as I know, in this country it has hitherto been given only by the mouth or rectum. When used subcutaneously, a comparatively small dose is followed in a short time by the usual constitutional effects, and, provided the solution be not too strong, and is injected deeply into muscular tissue, we have little reason to dread the formation of abscesses. My first patient, aged eighteen, had been subject to fits from the early age of five, occurring at first only after long intervals of a year or more, but as she grew older they became greatly increased in frequency and severity. In January, 1877, she had seven consecutively, and in April eleven in one day. About this time, though still unmarried, she became pregnant, and fretted very much in consequence, and for some days before her confinement, which took place prematurely about the sixth month, remained, I was informed, without any solid food whatever. On Friday, March 25, she was seized with convulsions, which recurred, to use her mother's expression, every five minutes, but I was not brought to see her till about 12 p.m., at which time a small putrid foetus had been expelled without any loss of blood. The pulse and respiration were both very rapid, and only a few minutes interval between each fit. I sent for some chloral, and in the meantime, as the urgency of the case was so extreme, began the treatment by chloroform inhalation, which had a marked effect in warding off the attacks. Shortly afterwards 5 grains of chloral, dissolved in about 30 minims of water, was injected in the thigh, and a similar quantity in about half an hour afterwards. At this time the pulse was 144, and the temperature 100°. The subsequent seizures were of brief duration, lasting only from thirty to sixty seconds, instead of as before, three or four minutes, while the great diminution in their frequency gave further proof of the improvement in the patient's condition. Three more 5-grain doses of chloral was given at intervals of about an hour—the whole amount being 25 grains. The last attack was noticed at 1 p.m. on Saturday, and was indicated only by enormously rapid and laboured respiration, attended by slight uneasy movements in the hands and feet. At 2 o'clock morphia $\frac{1}{3}$ -grain was given, and subsequently some beef-tea and brandy at suitable intervals. All the rest of the day she lay perfectly unconscious, the respiration varying from 35 to 50 in the minute, and the pulse from 130 to 140, being at one time so hard to feel that recourse was had to the injection of ether, as recommended by my friend, Dr. Macan, and

with the happiest result. 11 p.m.—Urine examined and found healthy; respiration very stertorous; pulse very weak.

March 27.—10 a.m.—Respiration 42, free from stertor; pulse 136. About noon it was noticed that deglutition was accomplished with great difficulty and pain, and soon afterwards she obstinately refused to swallow anything, and all nourishment had to be given by the rectum.

March 28.—Pulse very weak and compressible; respiration carried on through the nose; uneasy motions of the head and extremities, noticed yesterday, have ceased; has begun to swallow nourishment again, but with great difficulty; temperature $104^{\circ}3'$. As an enema failed to move the bowels, 5 grains of calomel were administered, and a turpentine stupe used to relieve some bronchial irritation which existed. She persistently refused to swallow any medicine, so I injected some quinine subcutaneously.

March 29.—About noon began to shake her head from side to side, at the same time making most horrible contortions. Respiration at this time 48, and pulse 140. As the bowels had not been moved, a turpentine enema was given, but without any effect until aided by a draught of that invaluable beverage known in the Rotunda as B B, otherwise Mist. Sennæ co. A most careful examination of the chest showed that there was no disease present, except slight bronchitis, quite insufficient to account for the very rapid respiration.

Her general condition improved somewhat after the action of the medicine, and the next day, for the first time since her illness began, she spoke a little. Nourishment and tonics, as well as whiskey, had been freely given as soon as the patient could be induced to swallow, but, notwithstanding, her convalescence was very slow indeed, principally owing to the deplorable condition of her mind and body before her confinement.

The second case of convulsions in which I adopted this treatment was admitted to the Rotunda Hospital some hours after the birth of a stillborn child. She was a primipara and unmarried; no seizures occurred till after delivery, and they were then neither severe nor frequent; 4 5-grain doses of chloral were administered with most satisfactory results. This patient's urine was very dark in colour, and became nearly solid when tested for albumen. I should add, that in this case also a little chloroform was administered by inhalation until the effects of the chloral injection were manifest.

Dr. Purefoy added that in the cases that he mentioned the treatment was begun by giving chloroform. In the first case the convulsions were very severe, and in using the chloral he was experimenting. The effect of the medicine was very marked in both cases.

Dr. MACAN thought the Society was greatly indebted to Dr. Purefoy for bringing forward these two cases of puerperal convulsions, treated by

the subcutaneous injection of hydrate of chloral, which was an entirely novel method of treatment in this country. He had found it mentioned by Dr. W. L. Richardson in a paper on the use of hydrate of chloral in labour, read at the first annual meeting of the American Gynæcological Society. He says, however, that the treatment had been abandoned on account of its causing troublesome and painful abscesses. Dr. Purefoy, by using a weaker solution and injecting it deeply into the tissues, seems entirely to have overcome this objection. Professor Liebreich recommends 7 grains as the proper amount for subcutaneous injection, and of course all medicine administered in this way acted more quickly and more powerfully than when given either by the mouth or rectum. He did not think, however, that Dr. Purefoy's two cases were quite satisfactory, for chloroform was given in both before the injection of the hydrate of chloral. Moreover, the first patient had been subject for years to epilepsy, and before she became pregnant had on one day as many as nine fits. Another circumstance which tended to confirm this idea was the absence of albumen from the urine. The case was also unusual from the period of pregnancy when the convulsions came on, for he gathered from what Dr. Purefoy had said that she had only been a comparatively short time pregnant. The second case was also not conclusive, for the convulsions did not come on until after delivery, and such cases were, as a rule, by no means so serious as those in which the convulsions appeared during labour, and were the most amenable to other methods of treatment. However, when in future he (Dr. Macan) met with a case of puerperal convulsions he would not hesitate to treat it by means of the subcutaneous injection of hydrate of chloral.

DR. MORE MADDEN.—The paper which has been read is one of great importance, and I think Dr. Macan's observations are of great interest also, although I do not myself agree with their views. Several years ago I read a paper stating that I had tried, as others have also done, the administration of hydrate of chloral in puerperal convulsions. I mention this because Dr. Macan has stated that if a case of puerperal convulsions should occur in his practice, he would think it right to give a fair trial to hydrate of chloral. I believe that if the case were not one of mere hysterical convulsions, which would get well whether hydrate of chloral or any other medicine were given or not, but a case of true puerperal convulsions, and that if he were to rely upon the hydrate of chloral while the patient was dying of the disease, that event would not probably be averted by this treatment. If the experience of those who have tried any remedy be altogether ignored, and if gentlemen start up at the end of some years to try the same experiment again, it will probably prove a waste of time and labour. We have a certain way of treating puerperal convulsions, and a tolerably certain way of curing them; and if we disregard this, and proceed to try experiments in one of the most

serious complications of labour—one of the most serious disasters in fact that can occur to a pregnant woman—and spend time in trying hydrate of chloral and other remedies of that kind, we may possibly incur the reproach of not having made ourselves fully acquainted with the literature of the subject.

DR. M'CLINTOCK.—I must say in all candour that I do not see the cases Dr. Purefoy has brought before us, when impartially looked at, justify any inference as to the value of chloral in the treatment of eclampsia. In the first case the patient appears to have remained for some days in a most critical state, and it is questionable whether the case was really one of puerperal convulsions at all or not. The convulsions occurred at a period of pregnancy when true eclampsia seldom appears. The patient, moreover, was in a most critical state for several days, and narrowly escaped with her life; and I do not see how we can attribute her recovery to the efficacy of the chloral. In the other case I do not think that the chloral had any decided influence one way or another. The convulsions occurred *after* her confinement, and she underwent other modes of treatment. I feel obliged, however, to Dr. Purefoy for bringing these cases forward. They show, at all events, that chloral may be administered hypodermically without any unpleasant results; constitutional or local; and that is a matter of some value. If I had a bad case of convulsions which resisted other treatment, and that the fits were very severe, I would certainly try the chloral subcutaneously. I must confess, with Dr. More Madden, that there are other modes of treatment which I would certainly put in practice before resorting to chloral.

DR. DENHAM.—We should never lose sight of the varied causes that may induce puerperal convulsions. In the case, for example, of a young plethoric woman with rapid pulse, flushed face, constipated bowels, and a variety of other symptoms, it is quite clear that the treatment should not begin with chloral. Other principles which have been laid down for the treatment of puerperal convulsions should first be brought into operation; and I do not hesitate to say that if a case such as I have just described were to come under my care, I would take a good dash of blood from the patient's arm in the first instance, and also take care that her bowels were fully and freely purged; and after those two points should have been gained, I believe that the way would be paved either for an anæsthetic such as has been spoken of, or for the administration of chloroform. I believe that in the case of a woman suffering from puerperal convulsions, injected chloral would act more rapidly than chloral given by the mouth. I remember carrying out a series of experiments in the Rotunda Hospital with chloroform at a time when chloral was not known, and the effect of it was to diminish the number and severity of the fits; at the same time I have seen the convulsions come on while the woman was actually under the influence of chloroform. I do not believe that two or three, or even

ten cases such as have been brought before us, would warrant a change from the old standard modes of treatment. I have not the slightest doubt that Dr. Purefoy's treatment was judicious for the cases that he had to deal with, but I do not think either of them were what we could fairly call cases of eclampsia. Dr. Macan thinks that convulsions coming on after delivery are less dangerous than those that occur during the stages of pregnancy. I regret to say that my experience is rather different from that. I look with much more apprehension upon convulsions occurring after than before delivery. Of course the case may be to a certain extent modified when the woman has been delivered and has lost a quantity of blood, but my experience has been that convulsions after delivery are on the whole more dangerous than when they occur before it.

SURGEON-MAJOR JOHNSTON.—Allow me to mention a case in which convulsions occurred in a somewhat peculiar way a few days before confinement. The woman was in the lying-in ward of my hospital. I was coming out of the ward when her husband met me and asked what was the matter. I told him his wife had had very severe convulsions. He rubbed his hands and said—"Oh, don't do anything to her, and she will have a baby in three days." I, accordingly, gave the woman no treatment at all, but waited; and exactly at the end of the three days she had her baby. I afterwards learned that the same thing had occurred several times before.

DR. PUREFOY (in reply).—I think Dr. Macan's observations, especially those as to the doubtful nature of the first case, are entitled to due weight. Still I think I am right in holding that the absence of albumen from the urine does not prove that the case was not one of puerperal eclampsia. I know that chloral has been injected subcutaneously in England long before I did it. The second of the cases I have submitted was not by any means a severe one; at the same time I have seen long-continued and severe convulsions lasting over two days, and not coming on until after delivery. I still think that convulsions occurring after delivery are not at all so serious as convulsions occurring before delivery. In reply to Dr. Macan's question, the first patient had reached about the sixth month of pregnancy. Not much more than a year ago I saw a case of puerperal convulsions in which the patient was quite insensible, and I did not think she would recover. She was stated to be only in the sixth month of her pregnancy, and from the size of her abdomen I think that was the case. That patient's recovery was due to venesection. I believe that chloral can be injected without abscesses ensuing from the use of it. I was greatly impressed with the benefit that resulted from the use of the chloral in the first case.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

President—EDWARD HAMILTON, M.D.

Secretary—E. H. BENNETT, M.D.

Mechanical Hyperæmia of Organs.—DR. J. W. MOORE said: About the 12th of January, a man, aged seventy-three or seventy-four years, was admitted, under Dr. Ormsby's care, into one of the surgical wards of the Meath Hospital. He had been suffering for a considerable time from obstinate eczema of both legs. After he had been in the hospital for a day or two, he was attacked with sudden palpitation of the heart. His heart was acting feebly, beating from 140 to 160 per minute. The pulse was extremely weak and fluttering. No cardiac murmur was audible. Bronchial râles were heard all over the chest. There was very considerable and universal anasarca. The diagnosis we ventured to make at the time was that he was either the subject of fatty degeneration of the heart, in an advanced stage, or that he had mitral regurgitation. The evidence of mechanical hyperæmia of the organs, in the absence of murmur, especially led to this diagnosis. The man died rapidly. The aorta presented a remarkable example of advanced atheromatous degeneration. This was specially noticeable at the commencement of the descending portion of the thoracic aorta. The innominate, left subclavian, and left carotid arteries were also diseased, but the ascending portion of the aorta was tolerably free from atheromatous change. It is dilated to a greater or lesser extent throughout its whole course. Along the base of the aortic valves, and also above their detached margins, atheromatous deposits are seen. At the right semilunar valve a dilatation is found, almost amounting to an aneurism; it occurs at the springing of one of the coronary arteries. The left ventricle of the heart is found to be considerably hypertrophied and dilated. There is very great dilatation of the mitral orifice, which freely admits the tips of four or five fingers. Similarly, we find the left auricle dilated; and the lungs, throughout almost their whole extent, are passively congested. Turning to the right side of the heart, we find the pulmonary artery very considerably dilated, and the chambers are also dilated. These effects of mechanical hyperæmia were not limited to the thoracic organs. The liver is rather enlarged, and, on section, presents a very good example of the nutmeg liver. The centres of the acini are exceedingly dark and congested, but their periphery is pale. On a recent section of the kidney, the capsules were found to be exceedingly adherent, and the

cortical substance pale in colour, contrasting remarkably with the deeply injected medullary portion. The kidneys, like the liver, had evidently for a long time been the seat of mechanical hyperæmia. The starting point of the hyperæmia appeared to be the extensive atheromatous changes leading to the dilatation of the ascending portion of the aorta. First there must have been hypertrophy of the left ventricle, and when that was no longer capable of overcoming the great strain, there was dilatation of that chamber and of the other chambers of the heart. The starting point of the atheroma might be found in the fact that the man was the subject of old standing gout. There is no doubt that the eczema of the lower limbs was gouty. He had been a butler in an old country family, and had, probably, become the subject of gout.

Commenting on the foregoing case, DR. HAYDEN observed that whenever consecutive dilatation occurs leading to valvular incompetency, the tissue of the heart will be found to have previously undergone either granular or fatty degeneration. He agreed with Dr. Moore as to the sequence of pathological events in the present case.—*January 26, 1878.*

Aneurism of the Innominate Artery cured by the Method of Tufnell.—DR. HEAD said: The case which I wish to present to the Society is one of an aneurismal sac that had undergone cure by the process upon which so much stress has been laid by Mr. Tufnell—viz., consolidation, by successive deposits of fibrin. The previous history of the case is short. In July last, the gentleman, who was leaving Ireland, came to thank me for my long attendance upon him, two years before, for a stomach affection. I had not seen him for these two years, and he told me that, during that time, he had suffered from intense neuralgia in the back and shoulder, along the back of the neck, and in the back of the head. For this he had been treated in various ways, and he had used large hypodermic injections of morphia, which gave him a good deal of relief; but, after some time, he was obliged to give them up, from the intense itchiness of skin which they caused. He then appeared to be well, and had not suffered much from neuralgia for some time. He was a Presbyterian chaplain in the army, and was going over to take charge of troops at Shorncliffe, in England. He mentioned to me that he had a little mark on his chest, beneath the right collar bone—a dusky spot, about the size of a five-shilling piece, as if he had got a bruise, and asked me to look at it. Upon examining it, I detected a distinct pulsation underneath, and came to the conclusion that he had a thoracic aneurism. But for the pulsation, however, it would have been impossible to detect an aneurism. There was no sign of pressure—no murmur, no difficulty of deglutition, no dilatation or irregularity of any vessel. There was no sign of interference with either recurrent nerve, and his voice was natural. The impulse of the aneurism was, if anything, slightly in advance of the heart's impulse, as if the commencement of the contrac-

tion of the heart acted upon the aneurism before the apex of the heart struck the walls. This I have observed in two instances of aneurism of the aorta. There was no double impulse, and no murmur or bruit. He complained of little or no pain, except a burning sensation. I told him that I thought he was not fit for duty, and explained to him, to a certain extent, the nature of his illness—that an effort should be made to cure him; and I suggested a consultation with Mr. Tufnell, who concurred in my diagnosis that it was an aneurism, and most probably of the arteria innominata. As I considered that this gentleman was likely to be benefited by the treatment which Mr. Tufnell has so ably advocated, we explained to him the nature of the treatment. He said that he would be glad to adopt it. He assumed the recumbent position the last week in July, and continued it until the middle of October. I had to leave town myself at the beginning of August, and Mr. Tufnell was then kind enough to take charge of the case. The patient took little or no medicine, and we did not give him any iodide of potassium; but, when he was under my sole care, and, occasionally, when his heart beat a little fast, he got small doses of aconite, which reduced the frequency of the pulse a good deal. The principal treatment consisted simply of the horizontal posture, absolute rest, and a minimum of liquids, his food consisting as much as possible of solids, so as to diminish the quantity of blood, and at the same time keep up its healthy condition. He bore his confinement very well; but, in the beginning of October, he began to show signs of great restlessness, and I thought it better not to confine him any longer. He began to get up, and gradually to go out; but became sleepless, and got into a state of great mental depression, fearing that he would be put on half-pay, and that he would be arrested for debts which really he did not owe. Mr. Tufnell and I now advised him to apply for additional leave of absence, thinking that, the aneurism being now very much consolidated, a little more rest might enable him to go back to his duty. He applied for additional leave, and on the very day before the occurrence of the unfortunate act which terminated his life, went before a Medical Board; the leave of absence recommended was granted, but of this he could not be made aware, as the proceedings are private. He was now advised to go to the country for change of scene, and his friends were taking him there, when, at the Railway Station at Kingsbridge, he became suddenly excited, ran away from them, and threw himself over the wall, which is twenty-four feet in depth, into the Liffey. He was not killed by the fall, but the water in the river was shallow at the time, and he was immersed in the mud. From this he was extricated as speedily as possible, and taken to Dr. Steevens' Hospital, and after lying there for about two hours, he died. I did not see him myself, but was informed that no impulse could be detected over the aorta, but he got a violent cough, which was characteristic of pressure. The Coroner directed a

limited *post mortem* examination to be made, which gave the opportunity of ascertaining exactly the site of the aneurism and the result. While undergoing treatment Mr. Tufnell and I came to the conclusion that the aneurism had undergone a great deal of consolidation, although to what extent exactly we could not tell, because there was still a strong impulse. It, however, gave the impression of a solid tumour striking against the sternum, and we could not feel any signs of lateral dilatation whatever. The centre of the impulse was about the edge of the sternum, between the cartilages of the first and second ribs, extending about an inch and a half in each direction; and there was also marked dulness on percussion. The heart was slightly displaced and pushed downwards, the apex beating between the sixth and seventh ribs, a good deal to the left of the nipple and over a considerable surface, and there was also some amount of dulness on percussion over the region of the heart. We came to the conclusion that the heart was slightly enlarged. Upon inspection by *sectio cadaveris*, a *solid* tumour was found, occupying the entire mediastinum, and firmly attached to the under-surface of the sternum and the cartilages of the first and second ribs. A portion of the sac of the aneurism being adherent to the under-surface of the bones, it was supposed at first that the shock of falling twenty-four feet had burst the sac of the aneurism, but it was found that there had been no rupture whatever, nor were there any signs of extravasation of blood. The heart itself was covered with fat, and to some degree enlarged, flat, and flabby. On opening the left side of the heart, the wall was found to be very thin and the cavity a good deal dilated; but the valves were perfect. When we opened the aorta, we found it very much dilated and altered by atheroma in a marked degree, with dilatation almost amounting to true aneurism. Upon slitting up the aorta, we found that the original aneurism occupied almost the entire of the arteria innominata. At the back of it the vessels were quite pervious—namely, the subclavian and the carotid on the right. Those on the left were also perfect; but the descending aorta was very atheromatous; and, at a distance of between three and four inches, there was another small aneurism, the size of a walnut. This we could not diagnose during life; but it, too, was all but filled up with fibrin. The principal interest in the case lies in the manner in which the aneurism was cured. The layers of fibrin are very firm and closely laminated, the layers of it being almost as thin as sheets of paper spread one over the other. It is a most interesting example of what may be done by the absolute rest and other items of treatment advocated by Mr. Tufnell—in fact, this aneurism was cured. This case ought almost to have been Mr. Tufnell's, for he had more to say to the treatment; but, as the patient was mine at the first and the last, I have laid the case before the Society. The age of the patient was fifty-six.

DR. BOOKEY said that the lungs were congested, and had a good deal

of frothy fluid in them, such as is met with in the lungs of a person who has been drowned. The patient, who was a heavy man, sixteen stone weight, had fallen twenty-four feet, and was found lying on his face. He died within two hours after he was received into the hospital, and breathed, it was stated, all the time he was there.—*January 26, 1878.*

Knee-joint Disease, with Disease of the Tibia.—DR. T. E. LITTLE exhibited for Dr. Thompson, Surgeon, County Tyrone Infirmary, a specimen of the above disease. Dr. Thompson's report of the clinical history and progress of the case is this:—"Anne L., aged three years, an apparently healthy child, was admitted into the Tyrone Infirmary, suffering from disease of the right knee-joint. *History.*—The patient had been well and strong until about thirteen months ago. She then got some strain of the knee, which rapidly swelled, and became very painful. The child grew very thin, got little or no rest, and had little or no appetite. A medical man was sent for, who opened an abscess below the tuberosity of the tibia, giving exit to a large quantity of pus, and affording much relief to the severe pain. The joint afterwards commenced to contract, the pain returned, and the patient rapidly lost flesh. *Symptoms on admission to the Infirmary.*—The face and body generally appeared fairly well nourished; the right knee was bent at an acute angle to the thigh; the joint was much swelled and extremely painful; fluctuation distinctly marked; there were a number of enlarged superficial veins over the joint. A sinus immediately below the tuberosity of the tibia communicated with diseased bone. The probe passed in so far that there could be little doubt it entered the knee-joint. The glands in the groin were slightly enlarged. The limb started at night, causing much pain. Latterly, according to the mother's statement, the child has been rapidly losing flesh. *Progress of Case.*—The limb was placed upon a splint, and efforts were made to extend it gradually. An abscess formed, pointed, and was opened about an inch to the inside of the first sinus, and exit given to a large quantity of thick, purulent matter, and relief was experienced for a time. Another large abscess, however, formed along the inside of the joint, but, apparently, external to it. The child getting rapidly weaker, I proposed amputation, to which proceeding the mother readily consented. The leg was, therefore, amputated at the thigh, by the double flap method, with circular incision through the muscles, at about its lower third. Ether was the anæsthetic employed, and it answered admirably. For reasons which are self-evident, excision was considered inadmissible." In presenting and commenting upon the specimen of the amputated limb, Dr. Little made the following remarks:—"On examination of the removed limb, I find the disease chiefly and primarily limited to the tibial aspect of the joint and upper extremity of the tibia. Reviewing the pathological anatomy of the limb, as to the conditions of (a) the soft parts, (b) of the

joint, (c) of the bones, we discover the following state of things:—(a) *Soft Parts*.—Two extensive sinuses exist over the region of the inside of the joint and upper part of the tibia. One of these presents a superficial fistulous opening below the internal tuberosity of the tibia, through which diseased bone could be reached with the probe during life, and which communicated with the joint, in the manner described further on. This sinus extended in the subcutaneous fascia upwards as high as the internal femoral condyle, communicating, in its course, with an opening on the anterior surface of the tibia. The other sinus, less extensive, had been opened below the head of the tibia, and extended also upwards towards—but not directly communicating with—the joint. (b) The joint was bathed in pus; the exterior ligaments and capsular structures are rather thickened; the cartilage is, in places, ulcerated and absorbed to a considerable extent—this condition, however, only slightly applying to the femoral or patellar parts of the joint; the cartilage has almost entirely disappeared from the facets on the upper surface of the head of the tibia; and the semilunar cartilages are almost completely removed—the floor of both articular facets being perforated, and communicating freely with the disease in the head of the bone; the synovial membrane, where remaining, is, in many places, thickened and pulpy, with numerous vascular fringes in its various interstices hanging into the joint. (c) *Bones*.—The condyloid region of the femur and the patella appear to be healthy, but there exists extensive disease of the cancellous tissue of the upper extremity of the tibia; here a considerable cavity is found, with several small pieces of necrotic bone lying loosely therein. This diseased cavity had opened both externally, through the surface of the bone, by means of a round, smooth, and, evidently, old-standing opening, with thickened osseous edges, of about the size of a pea, and which communicated with the subcutaneous sinus already referred to; and, internally, into the joint by a couple of free and irregular perforations of the articular facets on the upper surface of the tibia, which form the floor of the joint. *Remarks*.—The most important and interesting pathological question which, I think, this specimen suggests to our consideration is that of the primary starting point of the disease. The evidences are, in my opinion, in favour of believing this to have been in the cancellous tissue of the upper extremity of the tibia, and rather point to the articular disease as secondary to this. Amongst some of the reasons for this presumption I would mention the situation of the original abscess, and of the apparent primary focus of the disease (as described in the clinical history), the *early* occurrence of abscess, presenting externally, would be at least unusual at such an early stage of joint disease, the comparatively late period at which contraction of the limb supervened. These, with the anatomical characters of the parts—viz., the small amount of disease of the cartilages of the femur and patella, and the presence of extensive

excavation of, and necrosis of the head of the tibia itself, all appear to me to favour the view I have mentioned. [Since making the above report, I have heard that the case made an uninterruptedly good recovery.—T. E. L.]—*February 16, 1878.*

Ascites induced by Tubercular Disease of the Liver.—DR. BANKS said: On the 4th of July last, a little boy, aged ten years, was admitted into the Whitworth Hospital. We could not get an accurate history of the child, but we learned he had been ailing for a considerable time; and, on examination, we found that his abdomen was enormously distended, and the umbilicus was the size of a large walnut. He was labouring under ascites, and so urgent were his symptoms that, on the very day of his admission, it was found necessary to resort to the operation of tapping. He was a miserable, half-starved looking little fellow. The abdomen presented a remarkable appearance, being covered with a net-work of large veins, which communicated with those of the thorax. His skin was of a dusky, dirty-yellow colour, with a faint icteroid tinge, and the conjunctivæ were slightly yellow. He had had vomiting before he came in, and was weak and low. Five pints of straw-coloured serum were drawn off by the tapping, which was performed by an extremely small trochar. He experienced great relief from the operation, and his general condition was, to some extent, improved; but the fluid rapidly accumulated again—so much so, that in fifteen days it was found necessary to repeat the operation. On that occasion not so much fluid was drawn off. From that time until his death, which occurred on the 19th of November, there was never any considerable amount of effusion. After the second tapping, localised peritonitis set in, attended with very acute pain, which was greatly aggravated on pressure, which, however, yielded to treatment by opium. There was a good deal of distension, but it was chiefly due to flatulence, there being evidently very little fluid. His state fluctuated from that period. Sometimes he improved; but diarrhœa occurred at intervals, and his stomach frequently rejected food. He lingered on until the 19th of this month, when he died in a state of extreme marasmus. On examining the liver, to which attention was naturally directed, it was found to be studded with globular masses of a white colour, which are not now nearly so large as they were in the recent state. Masses of a similar character were found in the peritoneum. Enlarged glands were found pressing on the vessels of the liver. Dr. Harvey was good enough to examine the liver. Time did not permit him to make an exhaustive examination, but he will do so at his leisure and make a report to the Society. Dr. Banks said that, having found cirrhosis in some cases at as early an age, he expected to find cirrhosis of the liver, notwithstanding the early age of the patient, all the symptoms indicating the existence of the disease. It should be observed that there was

no disease of the lungs. Dr. Harvey made the following report:—
 “The liver was referred to me with a view of determining the nature of the peculiar elevated whitish masses, which occurred, for the most part, on the surface of the organ, but which, as I afterwards found, existed also, in considerable numbers, throughout its substance. On making sections, comprising one of the white masses, and the neighbouring liver tissue, I found the former were simply caseous masses, which presented no definite structure. These I regard as degenerated tubercles; for miliary tubercles, in considerable numbers, and in all stages of development, were found studding the healthy-looking portions of the liver. In one of the preparations, which I now submit to the Society, we have a very beautiful specimen of well-formed miliary tubercle close to an imperfectly-formed caseous mass; while some of the other specimens show masses of small, round (lymphoid) cells, which, no doubt, are commencing tubercles. Accordingly, I have no hesitation in reporting the case to be one of caseous tubercle. The liver shows some increase of connective tissue, but there is nothing that could be called cirrhosis. Masses of indurated lymphatic glands existed in the porta, and pressed upon the vein.”—*February 16, 1878.*

Melanotic Carcinoma.—DR. BENNETT said: The specimens I have here were taken from the body of a boy of about eleven years of age, whose case, although in many respects an ordinary case of malignant disease, still, I think, presents features sufficiently exceptional to deserve its being placed before the Society. On the 15th of last June the boy was admitted to Sir P. Dun’s Hospital. He was, to all appearance, perfectly healthy, and was ruddy, fat, and of full size for his age. At that time he was able to walk without lameness; but what he complained of was a lump in the popliteal space. On examination we found the whole natural cavity of the popliteal space entirely obliterated by a somewhat conical projection in the centre, enough to make us certain that we were dealing with some outgrowth which was not a swelling from any accidental cause. There was no pain in the tumour. The boy walked with a slight embarrassment of the motion of the tendons, and from the fact that he could not straighten out the limb completely, but there was no lameness from any kind of pain. His own account of the matter was that about three weeks before that date he had been struck by a hand-ball in a playground, the blow taking effect in the popliteal space. On further examination of him, I found in the groin a large globular tumour about the size of a racket-ball. Evidently the lymphatic glands of the groin were distended far above anything that we see in an inflammatory swelling. The enlarged glands were perfectly painless and adherent, and the contour of the glands could be made out as clearly as possible. Evidently the enlargement was not inflammatory,

but was from some new growth of the glands. His mother positively assured me that less than a week before his admission there was nothing whatever the matter with him, and that he had no swelling or tumour of any kind; and he himself said that up to a fortnight at least before that he knew nothing whatever of having any ailment. The whole limb was slightly swollen. So far it was hard to read the case. Not satisfied with the examination of the popliteal space and the groin, I made a further careful examination, and found that though the sole of the foot was at this time concave, still there was a tendency in it to become flattened down from the usual arched form, and that the structure of the sole of the foot was intensely hard. The fasciæ in the middle of the foot were as tight as possible. In spite of the history the boy gave, that the blow in the popliteal space was the origin of the affection, I was forced to diagnose a malignant tumour in the sole of the foot, developed immediately under the plantar fasciæ. There existed a glandular enlargement of the popliteal space, and a second glandular enlargement of the groin; and so clear were the indications, that it was impossible to avoid the conclusion that the evolution of the three tumours was almost synchronous. Certainly the tumours in the groin and popliteal space seemed to have developed almost immediately; but the growth in the sole of the foot was not noticed until I drew the boy's attention to it. There existed, therefore, a growth of malignant character in the sole of the foot, and almost simultaneously with it the glandular growths in the popliteal space and groin; the latter increasing at a pace we rarely see in malignant disease—in fact, beating the original disease in the rapidity of their growth. The limb assumed the appearance of *phlegmasia dolens* without being painful. It was greatly distended with a form of œdema which did not pit, which was evidently produced by venous obstruction. The surface of it showed small varices in the minute vessels of the skin. So the case went on till October of last year. For a long time the boy's health did not suffer apparently in the least. At last, about the end of September, in consequence of the size and weight of the limb, he had to be in bed, and after a time rapidly broke down. From the time when he took to his bed, in consequence of inability to carry the mass about with him, until he died, which was less than a month, hectic and diarrhœa supervened. There were no other features in the case. No thoracic or hepatic disease could be detected; and, with the exception of his emaciation and the continuous enlargement of the limb and tumours, there was very little to notice. The tumour in the sole of the foot, however, which I regarded as the original tumour, in the end beat the two glandular tumours in its growth. The contour of the sole of the foot became extremely convex, and at last the fasciæ gave way, and a mass protruded through the under-surface of the sole. We have a measure of the depth of the sole from this vertical section. What you now see has been shrunk in spirit, but it shows about three

inches depth of sole at what ought to be the hollow of the foot. The tumour was fungated, and there was a great flow of sanæous serum from it, as is usually the case in malignant disease. It was the only one that ulcerated. On the *post mortem* examination some points of interest presented themselves with respect to the distribution of the disease. I have here a piece of the sole of the foot. It is greatly altered, but one of the most remarkable features about it is that the mass is all through studded or infiltrated with cysts, from the size of a grain of shot to that of a small pea. In many parts there are bands of dark black matter, the colouring of which was better seen before immersion in spirit than it is now. Each of the growths in the popliteal space has the same infiltration of minute cysts, and also the bands of black discoloration. Tracing the deposit up, we found the infiltration in the glandular regions of the groin up to Poupart's ligament, and there was an infiltration of black matter also in the abdomen, along the spine, through the mediastinum, and up to the cavity of the thorax. The only glandular body in the thorax that showed this degeneration was the thymus gland. We examined the lungs, liver, and other solid organs, but found no trace of secondary deposit in them, although there were a few spots of black pigment in the thymus. In the glands around the kidney you observe the colour better than in any other part. The glands all along the spine have shrunk to a minute size, and there are little specks of black tissue through them, instead of their being enlarged; so that when they became subject to the melanotic deposit from the original tumour the majority of them underwent a cystic degeneration and shrank. A thin section from the sole of the foot, viewed under the microscope, shows these melanotic cells ranged according to the pattern of a true carcinoma—that is, it was originally a true carcinomatous tumour coupled with melanotic deposit here and there without rule or order. A singular feature in the case is the limitation of the path of this matter up to the thorax without any deposit occurring in the lungs or liver, and only a grain or two of melanosis in the thorax. The glands of the mesentery and those in the passage of the opposite iliac vessels are perfectly healthy.—*January 19, 1878.*

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, April 20, 1878.

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhoea	
Dublin, -	314,666	779	760	41	7	6	1	33	23	16	31·4
Belfast, -	182,082	501	453	6	7	7	—	3	17	12	32·5
Cork, -	91,965	189	220	—	2	—	—	4	9	6	30·8
Limerick, -	44,209	119	104	1	—	2	—	—	1	5	30·5
Derry, -	30,884	65	68	—	—	—	—	—	2	—	28·8
Waterford, -	30,626	46	94	—	4	—	—	15	—	1	40·0
Galway, -	19,692	31 ?	20 ?	—	—	—	—	—	1	—	17·3 ?
Sligo, -	17,285	20	33	—	—	—	—	—	—	—	24·8

Remarks.

In nearly all the towns the death-rate was higher than in the preceding period. It was particularly high in Waterford, Belfast, Dublin, Cork, and Limerick. In London it was 26·7 per 1,000 of the population annually, in Edinburgh 28·1, and in Glasgow 27·7. Omitting the deaths of persons admitted into public institutions from localities outside the registration district, the rate of mortality in Dublin was 30·4 per 1,000; within the municipal boundary it was 33·4 per 1,000. Zymotic affections caused 156 deaths, the average number in the corresponding period of the previous ten years being 132. Small-pox, whooping-cough, and diarrhoea showed an increased fatality; while fever, scarlatina, and measles were less fatal. At the close of the four weeks there were 203 cases of small-pox under treatment in the six principal Dublin hospitals. Of the 23 deaths from fever, 9 were ascribed to typhus, 10 to typhoid, and 4 to continued fever of undetermined type. In Waterford measles, and especially whooping-cough, were again very destructive to life. The deaths from small-pox in London were 241, or 58 more than in the

previous four weeks. Respiratory affections caused 199 deaths in Dublin (average of previous ten years=160·6), including 153 from bronchitis (average=121·7) and 24 from pneumonia (average=22·7). These untoward results were principally produced by the exceedingly inclement weather at the end of March and beginning of April. The returns for Galway during the week ending April 20 seem to be wanting.

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W., for the Month of April, 1878.

Mean Height of Barometer,	-	-	-	29·808 inches.
Maximal Height of Barometer (on 26th at 9 p.m.),	-	-	-	30·222 „
Minimal Height of Barometer (on 1st at 9 a.m.),	-	-	-	29·237 „
Mean Dry-bulb Temperature,	-	-	-	48·4°
Mean Wet-bulb Temperature,	-	-	-	45·6°
Mean Dew-point Temperature,	-	-	-	42·6°
Mean Elastic Force (Tension) of Aqueous Vapour,	-	-	-	·274 inch.
Mean Humidity,	-	-	-	81·0 per cent.
Highest Temperature in Shade (on 13th),	-	-	-	63·1°
Lowest Temperature in Shade (on 1st),	-	-	-	29·9°
Lowest Temperature on Grass (Radiation) (on 1st),	-	-	-	24·7°
Mean Amount of Cloud,	-	-	-	47·8 per cent.
Rainfall (on 16 days),	-	-	-	2·350 inches.
General Direction of Wind,	-	-	-	S.E.

Remarks.

The first few days were very wintry, with keen hoar-frosts at night, and showers of hail and cold rain by day. The weather then became milder, but remained rather changeable owing to the appearance of atmospherical depressions off the S.W. of Ireland and in the Bay of Biscay. Connected with one of these depressions an extraordinary although local downpour of rain occurred in London on the 11th—2·8 inches having fallen in a little over 12 hours. On the 13th temperature rose to 63·1° in Dublin under the combined influence of clear sunshine and a S.W. wind. Some days of truly April weather followed, and spring made considerable advances. There were a few beautifully fine bright days after the 21st; but a depression on the 29th caused a very heavy rainfall in Dublin—1·102 inches being registered in the 24 hours ending 9 a.m. of the 30th. On the whole, the month was a genial one. Hail fell on the 1st and 2nd, and sleet on the 1st. A solar halo was seen on the afternoon of the 7th, and a brilliant meteor to S.E. about 7 30 p.m. of the 2nd. The atmosphere was foggy on the 6th and 30th. The mean temperature was about the average, but 2·5 degs. above that of April, 1877.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

DIALYSED IRON.

DIALYSED iron was discovered by John M. Ordway, and was described by him in 1857. Messrs. Squire introduced dialysed iron in 1869, and it has also been imported both from France and Germany. It is a permanent neutral, inodorous liquid, of a deep red colour, but transparent in thin layers. It has none of the styptic taste so common and disagreeable in ferruginous preparations. With arsenical salts dialysed iron acts with great rapidity, even more quickly than the freshest preparation of the precipitated oxide of iron, which has heretofore been held to be the surest antidote for arsenical poisons introduced into the stomach. Dialysed iron may be administered for months together for all the purposes for which ferruginous preparations are usually exhibited. Becquerel says of it (and his observations are confirmed by many physicians of eminence) that it produces neither heartburn, diarrhœa, constipation, eructations—nor, in short, any gastric disturbance, and it never blackens the teeth. It is especially in anæmia, chlorosis, palpitations, chronic diarrhœa, gastralgia, dysmenorrhœa, &c., that its use is indicated. It is certainly tolerated well by some persons who do not bear other forms of iron. Dr. Weir Mitchell especially recommended it in a case of extreme exhaustion coming on in a professional pedestrian two weeks after a walking feat of fifty miles in ten hours. But its specific virtue is as an antidote for poisoning by arsenic. When taken into the stomach gelatinous ferric hydrate is produced. It also possesses the great advantage of being always ready for immediate use. The dose of dialysed iron for tonic purposes is from five to twenty drops four or five times daily. It may be taken in larger quantities, but to no advantage, as only a certain amount is absorbed into the system. From experiments recently made this amount is not more than from fifty to eighty drops per day. Of course, in cases of arsenical poisoning an excessive dose should be given at once. Dialysed iron is best administered by itself upon sugar or mixed with a simple syrup, which is free from an acid, as an acid admixture converts the preparation into a salt of iron. It can also be given in wine or coffee.—(*Virginia Medical Monthly*, September, 1877, and *Practitioner*, April, 1878.) The hypodermic injection of dialysed iron has been practised with success in a case of anæmia by Dr. Da Costa. Ordinary chalybeate preparations could not be given *per orem* in consequence of the disturbance produced in the

stomach and bowels. The daily quantity of iron injected was at first fifteen minims, diluted with water at the beginning, but subsequently given undiluted, and the treatment was continued for two weeks with a gradual advance to thirty minims per day. The solution used was neutral. No local inflammatory action was observed.—*N. Y. Med. Rec.*, April 13.

THYMOL.

THE essential oils of thyme, of American horsemint, and of the *Ptychotis ajowan* contain a substance, a homologue of phenol or carbolic acid, having the composition represented by $C_{10}H_{14}O$, and known as thymol. For more than two years this has been used by German surgeons, and is now being introduced among ourselves. It was discovered in 1719 by Caspar Neumann, examined chemically by Lallemand and Leonard Doveri, and first used to deodorise unhealthy wounds by Bouillon and Paquet, of Lille, 1868. In 1875 several German surgeons published investigations of its antiseptic properties, which are estimated to be from 4 to 25 times as powerful, under certain circumstances, as those of carbolic acid. Thymol is a crystalline, nearly colourless, body, with a pleasant odour and an aromatic burning taste. Its specific gravity is 1.028, and it melts at $44^{\circ} C$. It dissolves in 1,200 parts of cold water, 1 part of rectified spirit, 120 parts of glycerine, and in $\frac{1}{3}$ part of caustic alkalies. Fats and oils also dissolve it readily. It is prepared from the oils of either of the plants before mentioned, but pharmacists should beware of experimenting on English samples of oil of thyme, as but few of them are genuine, or, at least, contain any thymol. Its powerful antiseptic action, exceeding, under some conditions, that of carbolic acid, its small activity as a poison—about one-tenth of that of carbolic acid—and the absence of irritating effect when it is applied to the skin, all point to its use as a substitute for carbolic acid in the now well-known antiseptic treatment of surgical cases elaborated by Professor Lister. This substitution has been made with great success by Professor Volkmann, of Halle. For the spray solution, this gentleman uses a mixture of 1 part thymol, 10 alcohol, 20 glycerine, 1,000 water; but we understand that a solution in water only, which will not deposit, may be made by adding 1 part of thymol to 1,000 of hot water. For the gauze dressings used by Professor Lister others were substituted, made by saturating 1,000 parts of bleached gauze with a mixture of 500 parts spermaceti, 50 resin, and 16 of thymol. This prepared gauze is extremely soft and pliant, and, to use the words of the reporter, sucks up blood and the secretions of the wound like a sponge. The fibres of the gauze being impregnated with spermaceti, cannot, of course, become saturated with the secretions, so that they do not become stiff. Thymol has been used for various skin diseases by Dr. R. Crocker, but the results

of his experiments have not yet been published. As an internal remedy thymol does not seem to make much way. It has proved useful in diseases of the stomach accompanied by fermentation; and Mr. W. H. Stone reports in the *Medical Times and Gazette* that he has found it useful in cases of chorea, one form of which is St. Vitus' Dance. The present cost of thymol (3s. per oz.) is about five times that of the best carbolic acid, but as one part of the former seems to do as much work as 25 parts of the latter, the advantage of price is on the side of thymol.—*Chemist and Druggist*, March 15.

STAINING FLUIDS FOR MICROSCOPIC WORK.

DR. M. N. MILLER, of New York, gives the following information regarding the staining processes employed in the Histological Laboratory of the N. Y. University, which may prove useful to some of our readers:—I. *Borax Carmine*.—The original process given by Theirsch for the preparation of this most valuable agent is complicated and troublesome. Professor Arnold's method gives excellent results, and is simple in the extreme. A saturated solution of ordinary borax in water is first prepared. It is best to nearly fill, say, a pint wide-mouth bottle with water, adding the borax, in coarse powder to excess—allowing some of the crystals to remain undissolved. If good "No. 40" carmine be now added to the solution, it is immediately dissolved, and a beautiful scarlet fluid results. Continue the addition of the carmine until, after frequent agitation, it no longer dissolves, and a small portion remains at the bottom of the bottle mingled with the crystals of borax. After standing for twenty-four hours the clear supernatant fluid can be decanted. To this clear portion add about f 3 ij. of alcohol and f 3 i. of the U. S. P. solution of caustic soda,^a and it is ready for use. The method employed by Professor Arnold, which is in use in this laboratory, is to omit the alcohol, and evaporate the liquid to dryness, powdering the red amorphous mass thus obtained. This powder will keep for years, and when a staining fluid is required, it is only necessary to place from ten to fifteen grains in an ounce of water, to which about f 3 i. of alcohol is added. This compound will stain tissues that have been hardened in bichromate or alcohol, and is almost invaluable in pathological study. II. *Hamatoxylin*.—The expensiveness of this substance has led to several processes for using the logwood chips or extract. Dr. Miller has devised the following, which for results, he states, certainly leaves nothing to be desired, and possesses merit in the simplicity of preparation:—Take a pint bottle, as in the former process, fill with water, and add about an ounce of common extract of logwood, in coarse powder. Allow this to

^a The U. S. P. solution of caustic soda contains 5·7 per cent. of hydrate of soda, and its sp. gr. is 1·071. The B. P. solution contains 4 per cent. of hydrate of soda, and has a sp. gr. of 1·047.—[ED., PERISCOPE.]

remain in a warm place for twenty-four hours, with occasional stirring. After the expiration of this time add powdered commercial alum until the liquid changes from the muddy brown colour given by the logwood to a brilliant purple. The alum must be added until no change is produced. An excess of the salt will do no harm. Add about f ʒ ij. of alcohol, and, after decanting or filtering, it is ready for use. We usually omit the alcohol at this state, and evaporate to dryness, as in the borax-carminé process. The powder thus obtained is added to water when required. Three grains to the ounce of water will give a fluid which stains alcohol-hardened tissues in from ten to fifteen minutes. A solution containing ten grains to the ounce will stain very quickly. If it is desired to keep the solution, add f ʒ i. of alcohol to each ounce. III. *Picro-Carmine*.—Ranvier gives an exceedingly troublesome process for obtaining this complex substance. Dr. Miller's process consists merely in adding one part of a saturated solution of picric acid to two parts of the fifteen-grain borax-carminé solution. This mixture will, he believes, keep indefinitely, although he has never kept it longer than six months. A section of skin is one of the most beautiful objects prepared with the picro-carminé, as the epithelium of the glands and the muscles are stained yellow, while the nuclei of the cells and the connective tissue acquire the carminé colour. It is not, however, well adapted to tissues hardened in chromic acid. Sections, after having been stained in the borax carminé, should be washed in alcohol to remove the superfluous colouring fluid, transferred for a few moments to a saturated solution of oxalic acid in alcohol to fix the colour. The oxalic acid is then washed out in alcohol, when the sections are cleared up in oil of cloves, and mounted in balsam or dammar. If it is desired to mount in glycerine—which, by the way, meets with very little favour with Dr. Miller as a universal medium—the oxalic acid can be washed out with water instead of alcohol. Hæmatoxylin stainings are soaked in water for a few moments—to wash out the alum—transferred to alcohol, clarified in the clove oil, and mounted in Canada balsam or dammar varnish. Especial care must be taken that the vessels employed be clean, and that no acid comes in contact with the sections. If the fluid becomes muddy, or a scum forms upon the surface, it is only necessary to filter. Sections must remain in the picro-carminé generally for about twenty-four hours. They are then washed quickly, first in water and then in alcohol, after which transfer to the oil of cloves. The sections having been transferred to the slide, and the superfluous oil removed, the balsam or dammar (preferably the latter) is added, and the cover applied.

ESERINE AND PILOCARPINE IN THE TREATMENT OF EYE DISEASE.

DR. HENRY W. WILLIAMS has an interesting paper on the uses of these alkaloids in ophthalmic practice in the *Boston Med. and Surg. Jour.* of

March 14. Having given the results of his own clinical observation with eserine, he refers to the experiments of Weber, Mohr, Von Reuss, and de Laqueur, regarding its action upon healthy and diseased eyes. As regards the effects of eserine upon the cornea, the researches of these gentlemen seem to prove that the activity of the circulation is increased, that the pressure within the anterior chamber is lessened, that the action of accommodation is excited, and that the radius of curvature is shortened during its use. Increased activity in the blood supply, by rendering the cornea more highly vitalised, favours the removal of effete particles and the establishment of a process of repair; the diminished pressure upon the cornea (this pressure being itself a potent cause of ulceration) tends to limit the depth of the ulcer, and lessens the danger of perforation. Dr. von Wecker, of Paris, also believes that eserine prevents the pus from being reproduced in cases of corneal abscess, and in suppuration after cataract operation. We have thus a rational explanation of the benefit derived from the use of eserine in corneal affections. Referring to pilocarpine, the alkaloid of jaborandi, Dr. Williams says that in his own experiments, made with the chlor-hydrate of pilocarpine, the results obtained have differed a little from those produced by eserine sulphate, in the facts that less conjunctival irritation, less supra-orbital pain, and less spasm of the accommodative power seemed to be induced, while the contraction of the pupil and the temporary myopia corresponded in degree with those following the use of eserine. In these respects pilocarpine offers great advantages over eserine. It is, moreover, at present, less costly than eserine, and it does not, as does the latter, deliquesce on keeping. It is needless to say that these, as all other remedies, have their limitations of usefulness; in iritis, for instance, eserine and pilocarpine would doubtless be highly injurious, as tending to congest the already distended vessels, and as favouring the formation of adhesions between the iris and the capsule of the crystalline lens. M. Metaxa, of Marseilles, has also published the result of his experiences of the value of pilocarpine (*Gaz. Méd. de Paris*, 27 Avril). He has found it specially beneficial in iritis and irido-choroiditis of a rheumatismal nature.

THERAPEUTICAL ACTION OF MALT LIQUORS.

MR. J. J. COLEMAN, F.C.S., having lately been a sufferer from a serious illness, a prominent symptom of which was inability to digest food, was ordered, by Dr. Andrew Fergus, Hoff's Malt Extract. The usual dose—a wine-glassful twice or thrice a day—was taken with the result that food, which had hitherto escaped undigested, was assimilated, and the power of producing animal heat and storing up fat was increased. Other persons—thin, cold, or aged—were induced to become the subjects of experiment, and reported that it was far more sustaining than most alcoholic liquors. These facts led Mr. Coleman to make an extended

series of experiments, which he has embodied in a paper read before the Glasgow Philosophical Society, and which is published in the *Chemical News* for May 3. For the minute details of the experiments we must refer to that periodical. Analysis of the extract proved that, in composition, it closely resembled other malt liquors, differing chiefly by yielding a rather larger percentage of extract. In appearance it resembles porter. An explanation of the sensation of being lifted from a feeling of semi-starvation to the condition of being effectually warmed and nourished, seemed to be required. The 4 per cent. of alcohol in the extract, and other malt liquors, could not explain its permanent heating effects. The scientific experiments of Drs. Richardson and Edward Smith prove that an equal quantity of sugar has a more permanent power of warming, and popular experience confirms their statements. Now the diastase contained in malt is able to convert the starch of four or five times its weight of barley into soluble substances. Starch forms a very large proportion of our daily food, and it seemed possible that, by rendering this more soluble, Hoff's Liquid might produce the effects which had been experienced. Experiments were therefore instituted to decide the question, and it was conclusively proved that all malt liquors exert a more or less powerful solvent action on bread and other starchy foods, and that Hoff's Liquid possesses four times the power of Burton ale, and half as much again as London porter. This action is so great as fully to account for the good effects observed to follow their use.—*Chemist and Druggist*, May 15.

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